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The Journal of HEREDITY

A monthly publication devoted to Plant Breeding Animal Breeding and Eugenics

A Lily That "Breaks Its Neck" To Avoid Self-Pollination

JULY, 1926 **Vol. 17, No. 7**

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"BREAKING ITS NECK" TO AVOID SELF-POLLINATION

Frontispiece

A single flower of the Gloriosa lily showing the details of the arrangement of style and anthers, whereby the former avoids its own pollen by growing at more than right angles to the long axis of the ovary and also above the source of pollen. In spite of such elaborate precautions the Gloriosa Lily is self-fertile, or perhaps this is the reason that such an arrangement is necessary to insure cross-pollination.

AN UNUSUAL ADAPTATION FOR CROSS-POLLINATION

E. W. LINDSTROM
Iowa State College

A CLIMBING LILY
Figure 1

A plant of the Gloriosa lily in bloom, showing peculiar adaptation of the flower to insure cross-pollination; and also the curled leaf tips, another adaptation for a climbing habit.

THE Gloriosa climbing lily (*Gloriosa superba L.*) affords a noteworthy example of one of the devious experiments that nature often makes in providing a method for the reproduction of the species. Sometimes she favors extreme self-fertilization to attain this end as she does in the case of certain species of cleistogamous violets whose flowers are completely self-pollinated before the opening of the buds. Just as often does she provide some elaborate means of reversing this process so that complete cross-pollination is effected. In the Gloriosa lily the latter method is apparently called on, and the flower literally "breaks its neck" to escape its own pollen.

A glance at the photographs of this lily (Frontispiece and Figure 1) will reveal the striking fact that the style with its three-parted stigma grows abruptly at almost right angles to the main axis of the ovary, and projects sufficiently beyond the anthers on their long filaments to prevent the pollen from falling on its own stigmatic surface. Before the anthers dehisce the flower becomes pendant, and in this position the stigmas are not only projected beyond the nearest anthers but they are situated above the source of the pollen. Thus there is really a double insurance against easy self-pollination.

The perianth of the Gloriosa lily is gaily colored in scarlet and yellow. This may be an additional adaptation for effecting cross-pollination by insects, but the great length of the stigma and the fact that it projects so far from the reflexed, colored parts of the flower, would seem to prevent easy pollination by insects of the size of a honey bee or smaller. Only a large-sized butterfly or moth would be at all

effective in reaching the stigma with any parts of its body or wings.

In the bud stage, the style is relatively long, and is already bent sharply back from the tips of the three-loculed ovary. When the flower opens it merely assumes its right-angled position between two of the anther filaments, growing somewhat in length. The attachment of the style at the tips of the ovary is extremely interesting because it is so effective in producing a firm, right-angled union.

It may be worth noting in passing that the Gloriosa climbing lily possesses another interesting adaptation. This consists in the tendril growths of the leaf-tips, a splendid device for a climbing habit. These modified leaf-tips attach themselves with surprising strength, and will hold many times the plant's entire weight without uncurling. In fact it requires much force to loosen them by hand, and usually the leaf-tips actually break before uncurling.

Despite the extraordinary natural precautions against self-pollination in this lily, the species is self-fertile. At least seed pods with a full quota of developed seeds have been produced in the greenhouse as a result of controlled self-pollination.

Chromosomes and Rose Species

CHROMOSOMES AND CHARACTERS IN ROSA AND THEIR SIGNIFICANCE IN THE ORIGIN OF SPECIES, *by* C. C. HURST, PH.D. (Cantab.) F. L. S. Trinity College, Cambridge. (Reprinted from Experiments in Genetics, XXXVIII, pp. 534-550, figs. 169-175, Cambridge University Press. 1925.)

The author is the fourth who has treated extensively, within the past three years the chromosome conditions in one of nature's most polymorphic genera. He has studied Rosa from taxonomic and genetic as well as cytological viewpoints, and attempts to give a definite and uniform scheme of classification for this genus, based on certain septets of chromosomes and characters.

The Duplicated and Differential septet system into which all rose forms fit, subdivide the whole genus into a limited number of species, each characterized so definitely that a rose species becomes almost a mathematically determined organism.

According to the theory promulgated by the author our innumerable rose forms originated from one primitive northern decaploid species. By successive losses of the different septets of chromosomes and characters the lower polyploid and five simple diploid groups have come into existence, each adapted for its particular habitat. Such a view opposes the theory that places the diploid species in a primitive position and derives the polyloid forms from those having the basic chromosome number, seven.

The treatise should be very stimulating to students attempting to unravel the taxonomic, genetic and cytological problems in genera considered less complicated than the genus Rosa.

In conclusion one asks himself "Is a knowledge of the chromosomes of plants to be the Rosetta Stone of the botanist?" It seems too much to hope for or to expect.

THE FERTILITY OF THE STOCKS WHICH SUPPLY COLLEGE STUDENTS

S. J. Holmes

In a previous paper on *The Size of College Families*[1] the question was raised whether the classes of people who send students to college are sufficiently prolific to maintain their stock. From a study of family size among the parents of students of the University of California it was found that the average number of children in such families including deceased as well as living members was 3.66. This number is about thirty per cent less than the number of children in the families of the grand-parents (5.21). The general outcome of this study is in fairly close agreement with the results of an investigation carried on, in a somewhat similar manner, by Baber and Ross[2] with families sending students to the University of Wisconsin. There were included in the study of Baber and Ross the families of the uncles and aunts of the students as well as those of the parents. The average size of all the fertile families was found to be 3.35, while if the infertile families were included it was 2.81. The grandparents were found to have families averaging 5.44 children, there being a drop in family size for one generation of 38.42 per cent.

As was pointed out in my previous paper, the size of the college family arrived at by counting the number of children in the families of students tends to give an exaggerated picture of family size among the general class of people from whom our college population is recruited. The method gives simply the average size of families sending students to college. When we go beyond this fact and attempt to draw conclusions in regard to the fertility of the stratum of fairly intelligent, educated, and well-to-do people who furnish the bulk of our college students we are liable to fall into a statistical error. It is a well known fact that if we select a number of individuals at random and study the size of the families from which they are drawn, we should find that they usually come from families larger than the average among the general population. If families from one to ten children were equally numerous, we should, on the average, draw ten times as many individuals from families of ten as from families of one. Obviously, families with no children would not be represented in our collection at all. Studies of family size starting from collected groups found in schools, colleges, asylums, etc., tend, other things equal, to give an exaggerated number for the size of the family of the group dealt with. Failure to recognize this fact has not infrequently lead to erroneous conclusions.

In a recent paper entitled, *Are the Gifted Families in America Maintaining Themselves?*[3] Professor F. Lenz, of the University of Munich, has taken account of the tendency we have mentioned and has attempted to arrive at the true family size among the parents of gifted children. He derives his data from Terman's studies of ninety-one families producing exceptional children in California. The average number of children in these families was 3.40, which is an unusually high figure for the children of intellectual parents. In order to arrive at the true size of the family to be expected from parents of the kind considered, Lenz applies the method of reduction previously employed by Weinberg, Pearson, Greenwood, Yule, and Cattell in dealing

with other material. The families are arranged in order of size into those consisting of 1, 2, 3, etc., children. The number of families in each of these classes is then divided by the number of children in the corresponding family. The quotients so obtained are added, and the total number of families is divided by this sum. This gives the average expected number of children per family. Reckoned in this way the average size of gifted families based on Terman's material should be 2.18 instead of 3.40. This, however, does not take into account families having no children. If, in accordance with Cattell's findings for a somewhat similar group, this proportion is reckoned as seventeen per cent, the average size of family becomes reduced to 1.80, a figure far below what is necessary for maintaining the stock.

As my own material on the parents of college families presented the opportunity of studying the question with considerably more extensive data than those used by Lenz, I have gone back to my records and made estimates of family size according to the method just outlined. Tabulations of the number of children for families of various sizes had been made according to religion of parents and nationality of parents (whether native or foreign-born). Taking the group in which both parents are given as Protestant we may express our results in the following table (Table (I).

By making a similar calculation for the families in which both parents are Catholic we find the average number of children to be 2.89. Similarly, the families of parents both of whom are native-born average 2.38 children, while the families of parents both of whom are foreign born average 3.14 children.

These results, it should be noted, make no allowance for sterile marriages. Should the percentage of sterile marriages be as high as seventeen per cent the average family of native-born parents would be slightly less than two. For all the groups, even with a much less proportion of sterile marriages, the family size is insufficient for race perpetuation.

The data obtained by Baber and Ross do not permit making a similar reduction. The authors give, it is true, a tabulation of the number of families consisting of 1, 2, 3, or more children, but they have included in their results both the families of the parents of college students and the families of the parents' siblings. The first group is weighted for excess

TABLE I. NUMBER OF CHILDREN IN PROTESTANT FAMILIES

Number of children in family	*Number of families*	*Number of families divided by number of children*
1	453	453
2	852	426
3	792	264
4	541	135.25
5	356	71.20
6	222	37
7	131	18.71
8	76	9.5
9	55	6.11
10	25	2.50
11	17	1.55
12	3	.25
13	4	.31
14	2	.14
21	1	.05
	3,530	1,425.57

3,530÷1,425.57=2.47. Average Protestant family.

size on account of the method of selection. The second group is not so weighted. It is to be expected, therefore, that the family size of the first group would be larger than that of the second, and in fact Baber and Ross state that it is so, but without giving the statistical reason. The average sizes were 3.76 for the parents of the students, and 3.24 for the collateral branches, fertile families only being included. It is evident that using data collected by two different methods, one of which tends to exaggerate family size while the other does not, does not yield entirely reliable results.

On the other hand, can we rely upon the method of ascertaining true family size which was employed by Lenz? If we were dealing with families all members of which had an equal chance of becoming chosen as a member of a selected group, and if no more than one individual were chosen from each family, the method of reduction we have described would give us a fairly accurate picture of the distribution of families of different sizes in the general population. If for any reason all members of a family are not equally likely to be selected, or if there is a bias in favor of families of a certain size, then we cannot deduce the distribution of family sizes in our general class from our selected sample. This matter has been treated at more or less length in the controversy over the alleged inferiority of first-born children which has been carried on by Weinberg, Greenwood, Yule, and Dublin on the one hand, and Professor Karl Pearson on the other. If there is a bias toward selecting families of a given size, then if we start with a selected group without knowing the extent of the bias, there is no way in which the distribution of family sizes in the population from which the selection was made can be deduced from the family sizes of the selected individuals. Mathematics here will not help us. Experience alone will give us the clue.

Professor Pearson[4] has discussed more or less incidentally the case of the college family and has contended that large families would not necessarily be expected to send more students than small families. "Suppose," he says, "we asked every freshman who came to college to fill in a schedule, would there be any weighting at all of large families? Each member of a large family is not equally likely to come to college at the same time. They come, say at 18, and if a family has one representative at this age, it will rarely have a second. . . . The births of the members of a large family possibly spread over fifteen to twenty years, and they are not equally likely to become 'subjects' at a given epoch, or during a given short period of any process of recording from college or asylum or sanatorium." This is, of course, perfectly true; nevertheless it does not follow that families of ten are not, other things equal, much more likely to send students to a college than families of one or two. If individuals are liable to selection only at age eighteen, the small family is not likely to have an individual ready in any particular year, whereas a family of ten is many times as apt to have a qualified candidate at any particular time. Hence more large families would tend to be represented than smaller ones. This aspect of the question Professor Pearson seems to have overlooked.

One may urge, however, that in every large family some members are likely not to be living at any given time. If the size of the family, as in our schedules of college students, include deceased as well as living members, then the higher the proportion of deceased individuals the smaller the chance a large family has of being represented in a selected group.

The chance of having at least one deceased member increases with the size of the family, and this circumstance might be held to act as an offset to the weighting of larger fami-

lies in selected groups such as are found in colleges. On the other hand, there is a greater chance of smaller families not being caught at all because they do not last through as long a period as do large families, and the individuals in many of them may die before they become qualified for selection. On the whole, these various kinds of bias may come near cancelling each other in the long run.

It would be of interest to test the size of families among the parents of a selected group by the method used by Lenz with the actually observed sizes of fertile families of the siblings of these same parents. If the figures were found to markedly disagree one would suspect other kinds of bias than a purely statistical one. Our own expected figures fall short of those actually found by a little over one child per family. The selected families studied by Baber and Ross exceed those of their siblings by a little over half a child. The difference between our calculated figure and the one observed by Baber and Ross is indicative, I believe, of a counter bias in favor of the small family in our college material.

We have confined our discussion thus far to weighting of a purely statistical sort, but it is obvious that other potent factors may influence the size of families represented by college students. Smaller families may be better able to send students to college, whereas larger families on the same general level of culture would be unable to afford this advantage to their children.

If we would test the fertility of the general group of people that recruit our college population it is evident that the direct enumeration of the size of college families gives results which are too high. It is probable that the method used by Lenz gives results that are too low. If the college families do not represent a random sampling of their class for size then the method used by Lenz does not yield reliable conclusions. Even with the inexact data in our possession we do know enough about the situation to render it practically certain that the general stratum of population in California which yields the bulk of our college students is not a self-perpetuating group.

A study of the size of college families by Professor W. S. Thompson[5] has shown that family size varies considerably in different regions and with different occupations. Children tend to be numerous in agricultural groups and are fewer among parents engaged in trades and professions. Families are considerably larger in the South than in the North, and they are smallest in the Northeastern and Pacific States. Dr. Thompson concludes that "it is not true that the native stock in the educated classes is failing to reproduce, at least in the South and parts of the West, although in the Northeast and other parts of the West such is clearly the case." However, Dr. Thompson, who has secured his data from the families of students, did not seem to consider the fact that his method of selecting material tends to give an exaggerated representation of the larger families of the group drawn from. Just how much his figures would need to be scaled down is uncertain. If, as Baber and Ross's results indicate, they would require scaling down by about half a child per family, it would indicate that, outside of the South, most families, except those of the farming class, are below the reproductive level. There is no doubt that the birth rate has fallen since the period when the parents of college students produced their families. This kind of stock is largely of older American or northern European extraction and represents a stratum of people whose disappearance would cause a serious loss to the nation. Not many of them may be geniuses, but the bulk of them represent the more substantial, intelligent, thrifty, and enterprising element of the American people. More studies are needed in order that our knowledge of the re-

productive trend of this class be rendered more precise, for the fate of this class is a matter of most serious concern to our nation, since from this stratum of society comes most of our intellectual leadership.

Literature Cited

1. *Journal of Heredity*, Vol. 15, October, 1924.
2. Univ. of Wis. Studies in the Social Sciences and History, Vol. 10. 1924.
3. *Eugenical News*, Vol. 11, January, 1926.
4. On the Handicapping of the First-born. Eugenics Lecture Series, 10, London, 1914. (Reviewed in *Journal of Heredity* 6:332-336. 1915).
5. *Jour. Am. Statistical Ass'n*, Dec. 1925.

TABLE II. NUMBERS OF FAMILIES OF VARIOUS SIZES, CLASSIFIED ACCORDING TO RELIGION AND NATIONALITY

Catholic Families																					
No. of children in family	1	2	3	4	5	6	7	8	9	10	11	12	13	14							
No. of families	42	87	66	58	51	45	22	23	17	10	4	3	2	1							
Protestant Families																					
No. of children in family	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15						21
No. of families	453	852	792	541	356	222	131	76	55	25	17	3	4	2							1
Hebrew Familes																					
No. of children in family	1	2	3	4	5	6	7	8	9	10	11	12	13	14							
No. of families	26	58	35	37	19	15	14	6	1	6	1	1		1							
Families of Native-born Parents																					
No. of children in family	1	2	3	4	5	6	7	8	9	10	11	12	13	14							21
No. of families	426	792	682	451	282	186	100	56	41	17	7	7	2	2							1
Families of Foreign-born Parents																					
No. of children in family	1	2	3	4	5	6	7	8	9	10	11	12	13	14							
No. of families	54	112	145	108	101	81	56	33	21	17	10	2	4	1							
Families of Grandparents																					
No. of children in family	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
No. of families	371	642	1109	1325	1222	876	707	514	295	233	138	149	78	47	17	9	7	3	2	1	

Mule Raising in France

L'Ave, les Chevaux Mulassiers, et la Mule du Poitou, *by* Leon Sausseau. Doctor of Veterinary and Director of Veterinary Services of Deux-Sèvres. Paris, Librairies des Sciences Agricoles, 1925, pp. 342.

To those interested in either the economic or the biological side of the production of mules and hinnies, this book should be of considerable interest. It is concerned with the mule industry as developed for centuries in the province of Poitou, France. The historical side of the subject deals with the first appearance of the parental animals as indicated by their fossil remains, and with the first indications of their hybridization.

Tables are given of measurements of the horse and ass used in mule and hinny production at the present time. While no measurements on the mule or hinny are included yet the description of their characters, such as color, form, temperament, etc., in comparison with those of their respective parents, is of interest.

W. F. Dove.

INBREEDING VERSUS "ACCUMULATION OF BLOOD"

Defense of the Genetical Concept of Inbreeding Attacked by Baachuus-Jessen

H. J. Muller

IN AN article in the February, 1926, number of *Hereditas*, by Baashuus-Jessen, it is rightly pointed out that one of the chief tasks of the practical breeder must be the multiplication and concentration of the "blood" of particularly desirable "prepotent" individuals, by means of repeated matings of the same sire to related females of succeeding generations (preferably his daughters, granddaughters, etc.) The author fails to realize, however, that the task does not end here, since even the most "prepotent" individual arising in a population not strictly inbred is usually far from homozygous in all the genes that work in the directions desired, and hence even if a group of descendants could be obtained from him, in which each individual was completely identical with the original model animal in all respects, genetically as well as in appearance, that group would nevertheless continue to throw, in subsequent generations, many inferior types.

The initial "accumulation of blood" must therefore be followed by inbreeding (in the genetic sense), accompanied by rigorous selection, in order that stability of all the desired characters may be attained. The "rigorous selection" here referred to would dictate the abundant mating of a relatively few of the very best males, in each generation, to many of their sisters or half-sisters. They may even be mated to their daughters (thus slackening the inbreeding) until sons sufficiently well-developed appear, but eventually the sons (grandsons, etc.) must be used, to secure homozygosis. If in such inbreeding the ideal is not finally attained, because different desired elements have unavoidably become segregated into different lines or undesired recessives have come to light, then the best of these inbred strains must be crossed with each other, and, starting with the F_2 from this cross, the whole process outlined above must be repeated,—of "accumulating blood" of the best individuals and then inbreeding, and rigorously selecting. Each repetition of the crossing, selection, and inbreeding, will bring animals more nearly of the "optimum" composition.

To the average practical breeder who has little capital and time, and is anxious only for immediate results, the latter lengthy and expensive processes, involving repeated inbreeding, are of little interest as compared with the original and easy "accumulation of blood." Hence if our domestic animals (and cultivated plants) are to be improved as much and as quickly as they could and ought to be the work will probably have to be undertaken by large organizations, but only by such as are far-sighted and interested in future human welfare. It will be difficult, however, to raise the support necessary for these undertakings. Baashuus-Jessen's article is unfortunate, therefore, in confusing issues and in lending apparently scientific backing to the more short-sighted policies which already dominate breeding practice.

The unanalytical character of Baashuus-Jessen's arguments will be quite patent to the well-versed Mendelian (despite the misleading title of his article), but the show of terms and authorities used is likely to mislead many persons less technically trained, unless they are specifically

put on their guard against it. Thus, Baashuus-Jessen proposes a new measure of "inbreeding," but it amounts to no more than a bald statement of the fraction of the "blood" of a given individual furnished by some ancestor which may or may not have appeared several times in the pedigree. For instance, if A is both father and maternal grandfather of B—furnishing 50% of B's blood as father and 25% as grandfather—B is said by Baashuus-Jessen to be "75% in-bred." According to the same system every one of us would be 50% inbred with regard to our father and another 50% with regard to our mother, although 50% is regarded by Baashuus-Jessen as a fairly high degree of inbreeding. The immediate offspring, by self-fertilization, of the most heterozygous maize plant in the world would be 100% inbred, as much so as Johannsen's beans! No attention is paid here to the probable degree of homozygosis, but only this so-called "accumulation of blood" is considered. Having defined inbreeding to suit himself in this way, Baashuus-Jessen continues: "No greater biological importance can be attached to mating brother and sister than to mating half brother and sister. The problems of in-breeding and out-breeding will become much simplified if this line of thought be accepted." While the second statement is true, the first brings into glaring relief the fallaciousness of his whole attack upon previous inbreeding studies. Naturally, with this point of view, he regards Wright's calculations as incorrect.

It might be assumed that to challenge such intricate and rigorous mathematical work as Wright's Baashuus-Jessen must be armed with even sharper weapons, but on careful examination of his present paper, and of his previous paper in the same journal (1924), nothing appears in his support but vague assertion and unsupported "opinion." He obviously does not understand Wright's work, has not read it in detail, and does not know that, as Jennings, Fish, and others showed long ago, the attainment of homozygosis through continued close inbreeding is (barring balanced lethals or semi-lethals) not a matter of opinion or dispute, but as much a necessity, in its way, as the movements of the planets. The non-hereditary variation in coat pattern, of Wright's inbred guinea-pigs, shows, he says, "in my opinion, that uniformity which should have been a result of homozygosis, as automatically attained through the practiced inbreeding, does not exist." The time for opinion and discussion of such a matter has passed, and it is surprising to see the question raised in a journal devoted to technical genetics.

So many are the confusions of thought, and the errors of short-sightedness, in this article, that it would be unprofitable to pursue them in detail. Let one more suffice: ". . . culling of a strain for defects or weaknesses, by means of close in-breeding as advocated by Wright, cannot be carried out in practice. The remedy in this case is evidently out-breeding, not in-breeding." As was shown in the present writer's theory of balanced lethals (1917 and 1918) the latter procedure, if carried out consistently, must always be disastrous in the long run. It makes a flaring showing, for a while, by using up the hereditary capital and discarding the interest, thus taking refuge from the future in the present. It may well be termed "the ostrich policy."

In order to do full justice to Baashuus-Jessen's article, it may be repeated that his emphasis upon the value of rigorous selection, as practiced by means of the repeated use of especially well-favored sires, is indeed well placed. Some geneticists might have tended to forget that in practical breeding something more than mere homozygosis, as gained through selection of the best *pairs* of brothers and sisters, is desirable and possible,—namely, an abundance of the genes of the very best indi-

vidual animals, as gained through their extensive multiplication, when males, and subsequent inbreeding. Baashuus-Jessen is wrong, however, in implying that Wright was guilty of overlooking this fact in developing his theory of inbreeding, for Wright has shown (e.g. in a passage quoted by Baashuus-Jessen himself, p. 202, footnote) that he also appreciates the importance of the *kind* of genes present in the resulting homozygote, as distinct from the degree of homozygosis, and that he realizes the value of repeated use of a given sire, even to the extent of sire-to-daughter breeding, etc., in addition to brother-sister breeding, for securing the best kinds of genes in practical work. Wright's inbreeding formulae, to be sure, were at first intentionally designed for expressing the degree of homozygosis of whatever genes might be present, regardless of whether the latter were "good" or "bad," since this was a problem that had to be conquered separately, as a necessary and independent component of the whole theory of practical inbreeding. The formulae finally obtained in this way are so general, however, that they can also be applied easily to the more special cases of determining the proportion of genes, or of homozygous genes, from particular sources. Baashuus-Jessen, in specifically denying this other important element of the matter, developed by Wright, Jennings, Fish, Shull, East, Jones, etc.—the means and the value of securing homozygosis in the real genetic sense—has committed a far greater error than that of which he accuses the geneticists. Thus the obvious and previously recognized germ of truth that is reiterated in his paper should not blind us to the essential erroneousness of the paper as a whole.

Courtesy might have suggested a less harsh treatment of the article in question, were it not for the fact that its author obviously had not made himself conversant with the elaborate body of theory that he thought so lightly to attack. There is such great value, and often, too, such great difficulty, in the construction and establishment of a series of fundamental concepts like those worked out by Wright and by some of the other geneticists who have dealt with the subject of inbreeding, and it is at the same time so easy, by means of a few specious phrases, to bring such theories into public disrepute when their technicalities are not generally understood, that ill-considered attacks of the kind now being reviewed—whose very advantage lies in their unscientific character—deserve to be conspicuously exposed. Such ventures have recently become unfortunately numerous. The recent assault of Jeffrey upon the Drosophila work, published in *Genetica* August, 1925, represents another individual of the same hybrid and, shall we say, spuriously mutating? species.

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THE CAPSULES, SEEDS AND SEEDLINGS OF THE ORANGE DAY LILY

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FLOWERS OF ORANGE DAY LILY

Figure 2

All plants of the orange day lily (*Hemerocallis fulva*) are completely self-fruitless. Day after day the plants bloom profusely. The flowers are perfect, with a pistil and a set of stamens present and fully developed in each flower. But they yield no capsules and seed to careful self-pollination or to pollination from flower to flower among plants of the variety. The flowers merely wither to the very base and soon fall leaving conspicuous spurs with scars as shown above.

THE orange day lily (*Hemerocallis fulva*) exhibits one of the most obstinate cases of sterility or unfruitfulness known among flowering plants. For many years this plant has been grown widely as an ornamental in both Europe and America where in many localities it has escaped

and become naturalized. Yet all this propagation has been exclusively through the division and separation of the fleshy roots and rhizomes and never by seed reproduction.

In assembling day lilies for experimental study at The New York Botanical Garden, plants of the *fulva* type have been obtained from various widely separated points in Europe and America. These plants have all been identical in every character. They have all failed to set capsules to self-pollinations and for some of the plants such pollinations have been made in large numbers each year over a period of ten years. The flowers are perfect with a pistil and stamens present and apparently fully developed in each. Pollen is produced in abundance and in artificial culture it germinates well. But every plant of the *fulva* type has been completely self-incompatible.

All pollinations between plants of the *fulva* type have likewise failed to yield a single capsule. All the plants of the entire population of the *fulva* type are inter-sterile.

The facts noted above lead to the obvious conclusion that all the many plants of the orange day lily are merely parts of one original seedling. They constitute a *clonal* variety in distinction to species and varieties propagated by seeds and breeding true under such propagation. The clonal variety is, of course, very frequent among horticultural plants. It originates as a single seedling or bud variant which is ever afterward propagated vegetatively. When the original seedling is completely self-fruitless from any type of inherent sterility the many plants of the entire clon are inter-sterile for the simple reason that pollination between plants is the same as pollination from flower to flower on a single plant. The condition of complete sterility within a clon is common among horticultural plants when there is sterility either from intersexes or from self-incompatibility.

All the various named horticultural varieties of *Hemerocallis* are clonal varieties. Many of them are as fully self-fruitless as is the *fulva*. Nearly all of them, or perhaps all of them, are of hybrid origin and can hardly be expected to breed true to seed in case they do produce seed to selfing.

There seems to be some uncertainty as to the extent to which the true *fulva* is in cultivation in Japan and China and also as to whether it is to be found there in a really wild condition. The writer has received living plants of *Hemerocallis* from Professor Albert N. Steward of the University of Nanking, China and from Professor T. Susa and Professor Y. Hoshino of the Hokkaido Imperial University, Sapporo, Japan. These plants were all sent as *fulva* but they are obviously different than the true *fulva* and they differ among themselves.

It is to be noted that many plants grown from seeds obtained from China and that living plants obtained from China have very generally been found to differ from the *fulva* of long cultivation and have been given such varietal names as *maculata, Hupehensis* and *Cypriani* or have been called a distinct species (*H. Forrestii*). Probably all these forms should be included with *fulva* in a single species that is merely variable in nature. At least one of these varieties, the *maculata*, has yielded seed rather freely and has been used by Willy Müller of Naples, Italy, in crosses with *H. citrina*.

Eighteen of the plants received from Japan and China have bloomed. All but two appear to be self-incompatible but they produce capsules and seeds to certain cross-pollinations which is a behavior characteristic of the seedlings of many species having this type of sterility.

Fulva as a Seed Parent in Crosses

But the plants of the *fulva* clon have also been decidedly unfruitful in cross-pollination with other types of day lilies. For many years *H. fulva* and *H. flava* have been widely grown together in gardens yet there seems to be no

CROSS POLLINATION CAUSING PARTIAL DEVELOPMENT

Figure 3

Frequently capsules start to develop on the *fulva* day lily when its flowers are cross-pollinated with pollen of certain types of day lilies. But when such capsules are about one-third matured (see above) they nearly always wither and fall. The foreign pollen thus merely stimulates a partial development of capsules, an effect not seen in the self-pollination of *fulva*.

record that any of the plants of *fulva* have ever produced seed. From time to time collections of different species and varieties including the *fulva* have been grown together under conditions that favor cross-pollination by insects. For ten years plants of *fulva* have been thus grown at The New York Botanical Garden without producing a single capsule to the open pollinations that may have occurred.

Various breeders of day lilies have attempted to secure seed from *fulva* through cross-pollination by hand. One of these Mr. George Yeld[4] reports as follows:

> As to *H. fulva,* I have many times used pollen of it, but never with success, and it, moreover, refuses to seed. I have looked for seed of it in many places—in Auvergne, in Switzerland, in Northern Italy, near Naples, in Cicily, in the Lipari Islands, in Northern Asia Minor, in Trans-Caucasia;

CAPSULES PRODUCED BY CROSS-POLLINATION

Figure 4

(*Above*) Two of the capsules with seeds matured by plants of *fulva*. The one at the left is from cross-pollination with a plant from Japan, the capsule at the right is from pollen of *H. citrina*. As here shown the number of fully developed seeds in these capsules is small. Usually there are also some shrivelled seeds with half developed and dead embryos.

(*Below*) Five mature capsules produced by plants of the *fulva* day lily. Twenty-three capsules are all that have been obtained in over ten years of work during which 7,135 flowers of *fulva* were cross-pollinated.

SEEDLINGS OF FULVA

Figure 5

Including the single seedling recently grown by Amos Perry, these seedlings with four others not here shown are apparently the first and the only seedlings ever obtained from the *fulva* variety of day lily. These are all hybrids with *fulva* as a seed parent. The two at the rear have for their pollen parent a wild plant from China somewhat resembling the true *fulva*. The three in the middle row have as the pollen parent the well known *H. Thunbergii*, which has flowers of a lemon yellow color. The two at the front are from pollen of the rather recently introduced species, *H. citrina*.

but I have never yet found a single pod, and fairly persistent inquiries have failed to bring to light an instance of its seeding.

Evidently many similar efforts to secure seed of *fulva* have failed for there appears to be no successes reported previous to those recorded by the writer[1,2]. The first of these seeds obtained from *fulva* were from the pollen of *H. Thunbergii* and they did not germinate. Efforts to obtain capsules with viable seeds were continued with such success that at this date plants of the true *fulva* have matured 23 capsules containing a total of 70 seeds apparently fully mature and from this seed eleven seedlings are now being grown. Only two other authentic records of seed produced by the *fulva* variety have been found by the writer, either in printed records or through a rather extended correspondence. In a letter dated Nov. 18, 1925, Mr. Amos Perry, well known nurseryman of Enfield, England, lists a variety "Maggie Perry" as derived from the cross *fulva* × *fulva* var. *Cypriana*. In a letter dated Jan. 22, 1926, Dr. J. R. McLeland, of Pleasanton, Kansas, writes as follows:

> I have never been able to cross *Hemerocallis fulva* with anything that would do more than make it set empty seed pods, with two exceptions when I used pollen of *H. citrina* I got two pods with one seed each but neither germinated.

Among the various persons who have undertaken to breed day lilies there may be others who have obtained seed and even seedlings of *fulva* but of whom the writer has not heard.

But plants of the *fulva* do not readily yield seed to cross-pollination with the other species and varieties of day

lilies. It has been necessary to make many pollinations to obtain the few seeds secured at The New York Botanical Garden. Of 527 flowers carefully pollinated with pollen of *H. aurantiaca* only two mature capsules and four seeds were obtained and these seeds did not germinate. From 1,763 flowers pollinated from *H. Thunbergii* only nine capsules were obtained with 19 seeds of which six germinated. With pollen of *H. citrina*, 421 flowers of *fulva* have been pollinated but only eight capsules matured with 32 seeds of which three germinated. There were complete failures for 33 flowers pollinated from *H. Dumortierii*, for 121 flowers pollinated from *H. flava*, and for 570 flowers pollinated from the horticultural variety *lutcola*. Many cross-pollinations also failed with pollen of the double-flowered varieties *H. fulva Kwanso* and *H. fulva flora-pleno*. The *fulva* clon has been unfruitful to pollen of every one (51 in number) of the seedlings thus far used in the tests. Pollen of fourteen plants from Japan and China has been used in pollinating flowers of *fulva*. In one combination 105 flowers gave one capsule and five seed, from another involving 107 flowers two capsules and 9 seed were obtained and from another combination involving 75 flowers one capsule and two seed were had.

The total number of flowers of *fulva* cross-pollinated in all combinations is 7,135. These have yielded only 23 mature capsules and 70 seeds of which eleven have germinated.

Fulva as a Pollen Parent

The *fulva* variety is the pollen or male parent of certain hybrid seedlings some of which have appeared in the trade. The first of these of which the writer has record were from the cross *H. flava* × *H. fulva*, and were produced at the Botanical Garden of the University of Strasbourg, France. Six hybrids, presumably of this parentage were offered for sale in a trade list issued July, 1910, by Willy Müller, Nocera Inferiore, Naples, Italy. Through correspondence with Mr. Müller and with Professor Karl Killian of the University of Strasbourg the writer learns that several hybrids were obtained with *fulva* as the pollen parent, that some of these were crossed with *fulva* as the pollen parent but that during the recent war the breeding was discontinued and with the death of the head gardener (a Mr. Muller) and of Professor Solms Laubach the records of the parentage of the remaining plants were lost. Through the kindness of Professor Killian the writer has received from Strasbourg living plants of the *H. flava* × *H. fulva* hybrids and also of what is labelled as a hybrid between *H. graminea* and *fulva*.

The hybrids between *flava* and *fulva* obtained at The New York Botanical Garden were first reported several years ago[1,2] and again mentioned in a recent publication.[3] To the present time seven of these hybrids have bloomed. Six other hybrid seedlings have been obtained from the cross *H. citrina* × *H. fulva* and two others have resulted from using pollen of *fulva* on a hybrid between *H. flava* and *H. aurantiaca*. The pollen of the double-flowered variety *Kwanso* used on a hybrid of *flava* × *fulva* has also yielded two seedlings. But to obtain these seventeen seedlings many flowers have been pollinated.

Other seedlings with *fulva* as the pollen parent are to be mentioned. A recent letter from Amos Perry, Enfield, England, lists for sale the hybrid clonal variety "George Yeld" as a cross between *H. citrina* and *H. fulva*. Mr. B. Y. Morrison of the Bureau of Plant Industry, Washington, writes that he has seedlings obtained from *fulva* pollen on two plants of questionable identity Mr. C. Betscher of Dover, Ohio, states in letters received in November, 1925, that he has used pollen of *fulva* and *flava Kwanso* in securing certain hybrids but whether in controlled pollinations or not is not stated.

At The New York Botanical Garden the hybrids of *H. flava* × *H. fulva* and of the double cross (*flava* × *aurantiaca*) × *fulva* have themselves been crossed with *H. aurantiaca, H. citrina, H. Thunbergii,* the varieties *Florham* and *lutcola,* and with several plants from China and Japan so successfully that over 1,200 seedlings have been obtained all having the *fulva* as one or two of the grandparents. Most of these plants have not reached blooming age but most of those that have bloomed possess flower colors which show the *fulva* parentage. Flower colors similar to certain of the *fulva* offspring have, however, been obtained in hybrids between species and varieties other than *fulva,* so on the basis of flower color alone seedlings of unknown parentage are not necessarily to be ascribed to *fulva.*

Conclusion

The history of the cultivated day lilies known as *Hemerocallis fulva* may be reconstructed as follows: Perhaps more than a thousand years ago a Chinese gardener saw a single seedling day lily, either wild or in a garden, which he deemed worthy of propagation. Its habit of spreading by rhizomes made vegetative propagation simple and rapid. As the seedling happened to be fully self-incompatible, the condition of the great majority of all day lilies, vegetative propagation was necessary. In time the clon became widely cultivated even in Europe and America and continues to set no fruit to self-pollination or to pollination between the various plants.

It might appear from the decided fruitlessness of this clonal variety that the apparently perfect pistils of the flowers are only rarely perfect enough to function in fruit setting. The pollen is, however, abundant and gives good germination in artificial culture yet it has thus far functioned in only a relatively few of the many crosses in which it was used.

The day lilies as a group, the species, the varieties and the numerous seedlings already studied, are decidedly fruitless to self-pollination. Many crosses between them fail completely. In a relatively few cases individual seedlings and clonal varieties are self-fruitful. Certain crosses between self-sterile plants are highly productive of seeds. It is clear that the sterilities of the group involve chiefly incompatibilities, both self and cross, in the processes of fertilization. In such cases fertilization is selective and discriminative; the pollen and the pistils are highly functional in certain relations but not in others. Capsules and seeds are produced when compatible pollinations are made.

Thus far the *fulva* clon has been completely self-incompatible and in its crosses with other day lilies, both as a seed and as a pollen parent, the compatibilities are of a weak grade and only successful in a very low percentage of the flowers pollinated. It seems very probable that highly compatible cross relationships will be found, possibly with some of the seedlings now being grown or with plants that now exist in China or Japan, and that then the plants of *fulva* will yield capsules and viable seeds abundantly and at the will of the pollinator.

Literature Cited

1. Stout, A. B. 1919. Conference Notes for February. *Jour. N. Y. Bot. Garden* 20:103-105.

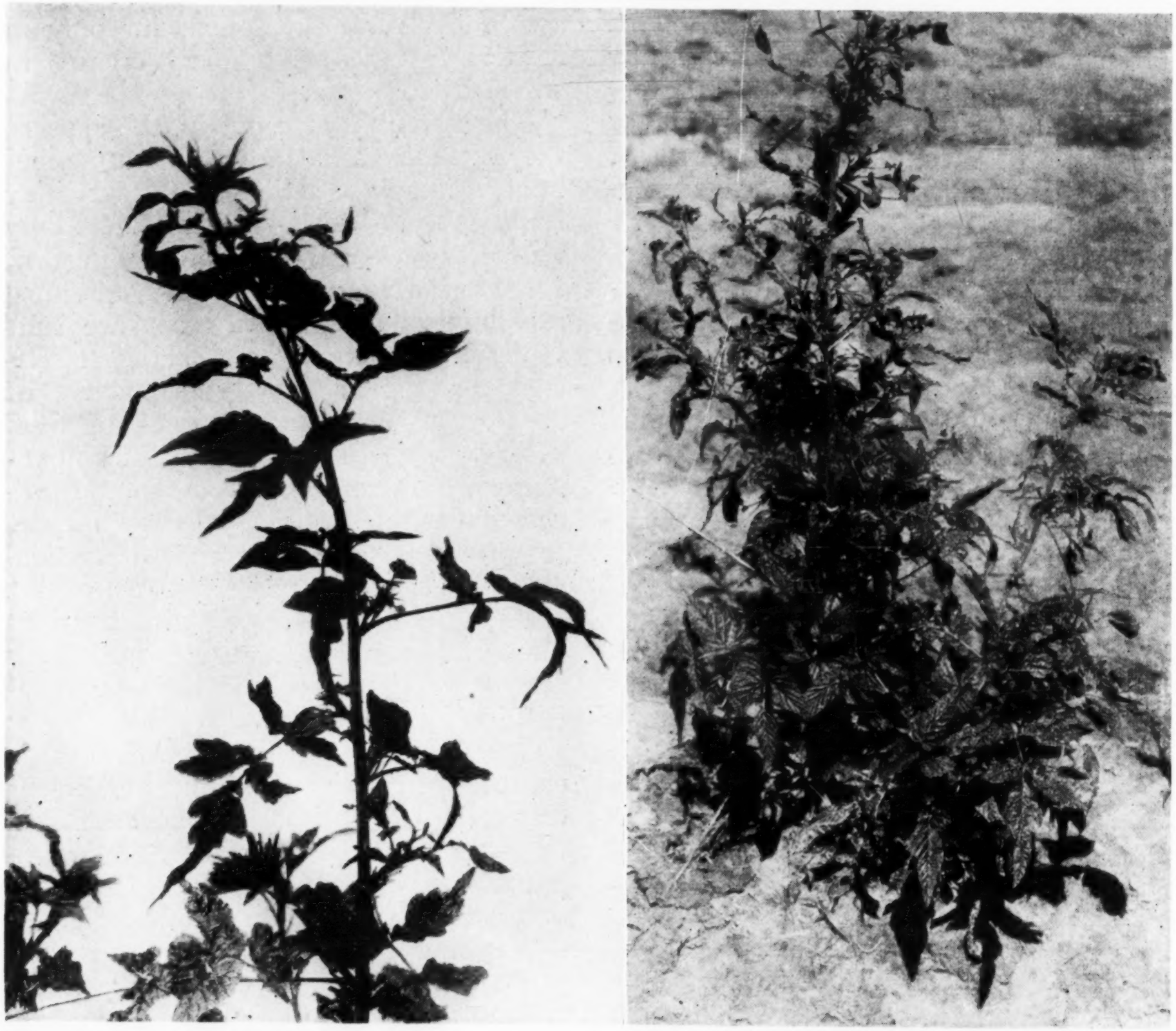
2. ———————. 1921. Sterility and Fertility in Hemerocallis. *Torreya* 20:57-62.

3. ———————. 1925. New Day Lilies. *Jour. N. Y. Bot. Garden* 26:169-178.

4. Yeld, G. 1906. Hybrids of *Hemerocallis. Rept. Third Int. Conference on Genetics.* London.

ABNORMAL GROWTH OF RASPBERRY

GEORGE M. DARROW



A B

RASPBERRY ABNORMALITY OF UNKNOWN ORIGIN

Figure 6

For several years one or more hybrid raspberry plants at the U. S. Horticultural Field State at Glenn Dale, Maryland, have appeared with the peculiar abnormal development shown above. Each year the plants as they appeared have been burned because they were worthless horticulturally and because of the possibility of their being affected with a virus disease. The leaves are somewhat mottled and are more or less laciniate, sometimes little except the midrib remaining. Though examined for mites, none was found. The bush shown in (B) was not dug when the abnormal growth first appeared and when examined on June 17, 1925, the shoot shown in (A) had grown from the larger cane of (B). Its tip appeared normal. Possibly the abnormal growth is due to a virus disease, the effects of which are masked at high summer temperatures.

INHERITANCE OF BLACK IN SWINE*

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THE history of the Poland-China and the Duroc-Jersey breeds of swine shows that much of the ancestry of these two breeds was the same or, at least, similar. In the early days of the breed, the Berkshire also appears to have been black-spotted on sandy body color. It is interesting to speculate as to the process of selection followed in arriving at the present-day colors of Poland-Chinas, Berkshires, and Duroc-Jerseys.

Reasoning from a genetic basis, the red of the Duroc-Jerseys may have been secured in either of two ways, depending upon the inheritance of black. One of these is by selection of animals with smaller and smaller black spots in each generation until the animals became self red. This would assume multiple factors for black spotting. The other way is by selection of self-red animals, which probably appeared in a fairly large proportion of the progeny of the spotted ancestry. This would require either a one-factor difference, or, at most, only a few factors. The study herein reported is an attempt to determine the genetic differences between Poland-China black and Duroc-Jersey red.

Previous Investigations

Lush[1], and Wentworth and Lush[2] crossed Berkshires with Duroc-Jerseys and found that the F_1 progeny were red with black spots. The F_2 crosses resulted in 75 per cent of the individuals having some black present and 25 per cent entirely lacking black. The body color varied from white to red. The amount of black, when present in F_2, varied from almost none to almost as much as in the parent Berkshire. They interpret their results on the basis of one pair of factors for black, two for reds and sandies, and, in addition, a diluting factor linked to black, acting on one of the factors for sandy.

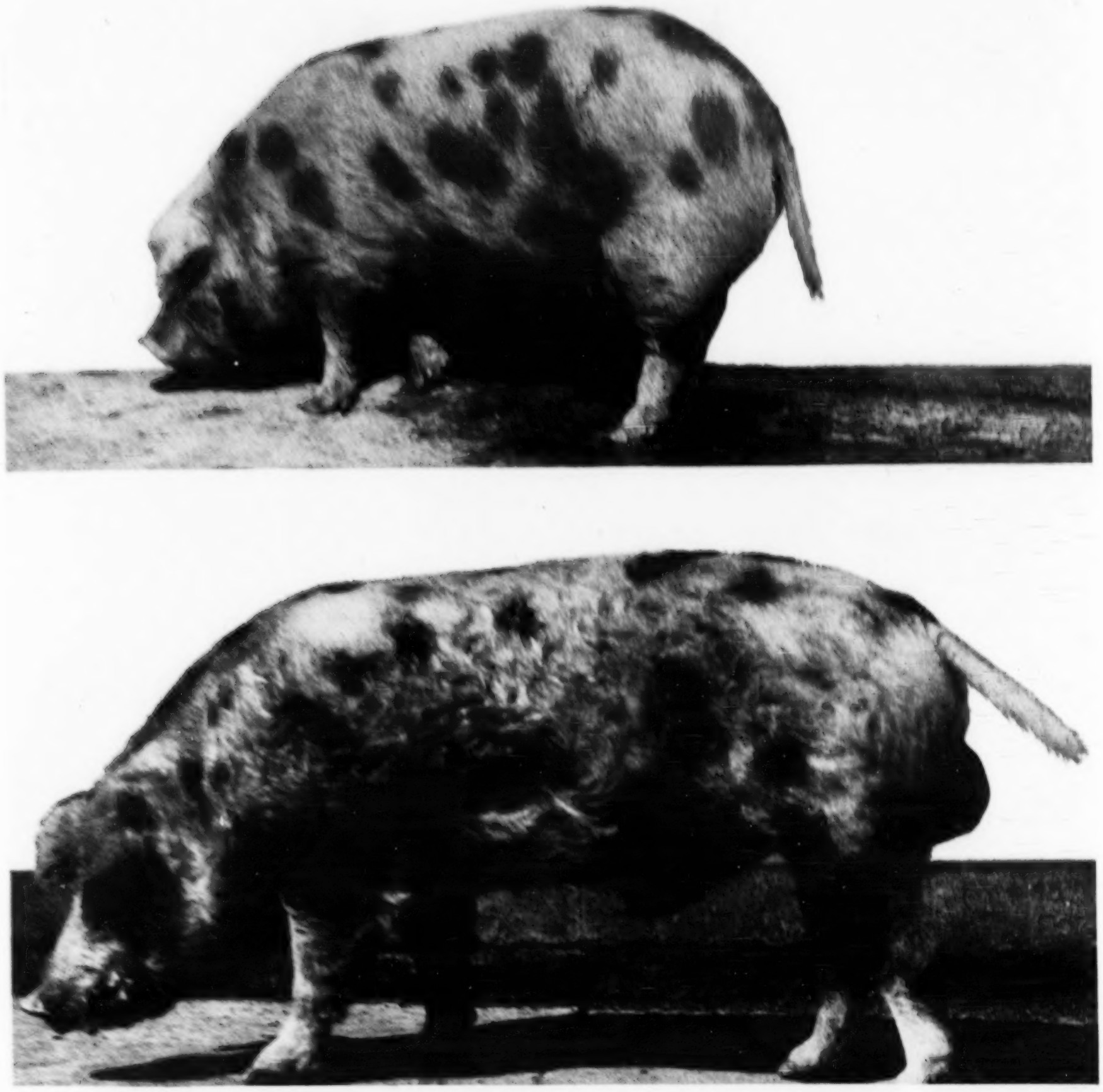
Wright[4] has reviewed the literature on color inheritance in swine and does not admit a single factor difference between the black of Poland-Chinas and Berkshires and the red of Duroc-Jerseys. He assumes that the differences are due to minor factors for restricting the black and intensifying the red, in the case of the red breeds, and extending the black and diluting the red in the black breeds mentioned.

Experimental Studies of Color Inheritance

One Duroc-Jersey boar was mated with Poland-China sows and gilts. One hundred and twenty-five progeny were born from these matings, all of which showed black spots. One litter of six was raised from the reciprocal cross. No material difference could be observed in results according to the two ways the cross was made. The body color between the black spots of the individuals of the F_1 generation was mostly red, but varied in different individuals, from almost white, through the shades of sandy, to red.

One F_1 boar, with about the average amount of black, and dark sandy body color, was mated to F_1 sows, and 16 litters of F_2 pigs were produced. These included 138 individuals. One hundred and seven of these had one or

*Papers from the Department of Genetics, Agricultural Experiment Station, University of Wisconsin, No. 63. Published with the approval of the Director of the Station and of the Chief of the Bureau of Animal Industry, U. S. Department of Agriculture. This work was carried on in connection with a study of hernia which will be reported in a separate paper. The hernia project was a cooperative investigation between the Departments of Genetics and Veterinary Science of the Wisconsin Agricultural Experiment Station and the U. S. Department of Agriculture.



COLOR PATTERNS IN FIRST GENERATION

Figure 7

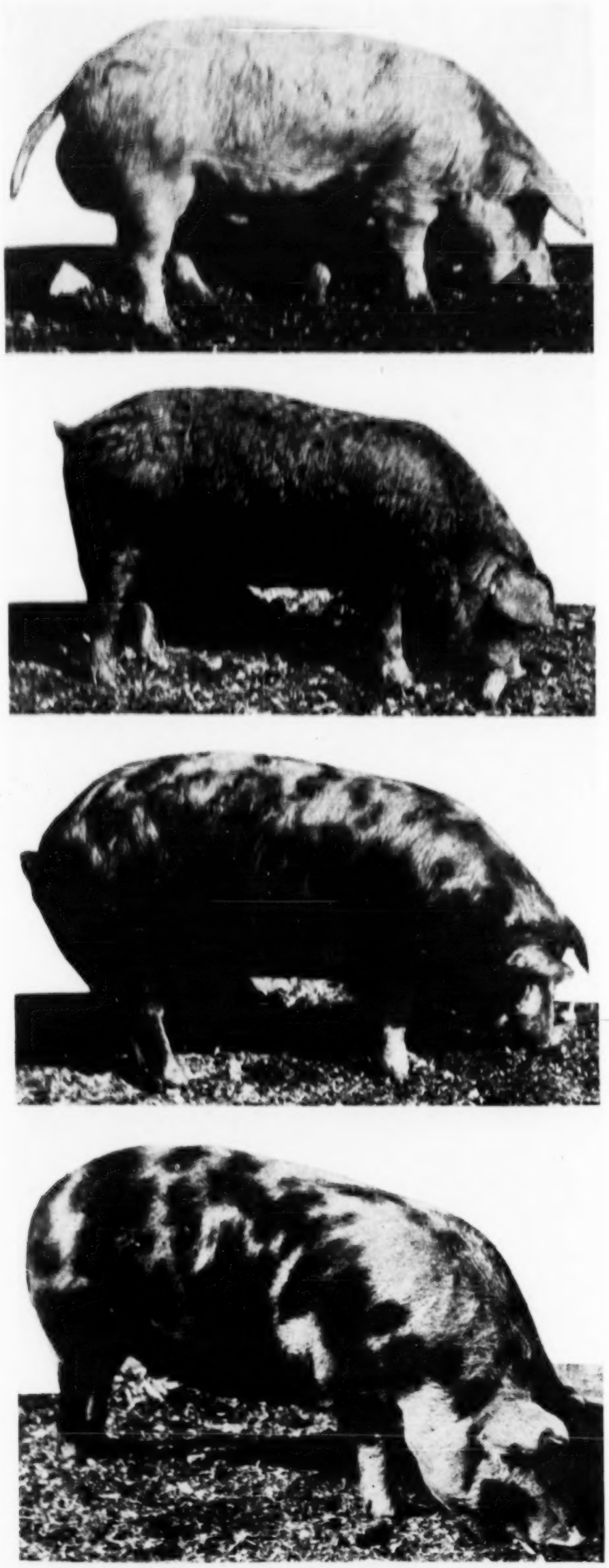
(*above*) Duroc-Jersey × Poland-China F_1 sow showing typical spotting with very light red body color. (*Below*) Crossbred boar by a Duroc-Jersey sire out of a Poland-China dam. This was the F_1 boar used in the breeding experiments. He shows about the average amount of black on F_1 individuals, while the body color is slightly lighter red than the average. The body color of F_1 animals is much lighter as they approach maturity than when they are younger.

more black spots, while black was absent on the other 31, as shown in Table I.

Thus, it is seen that the deviation is relatively small and the ratio is typically monohybrid. With one exception, each F_1 dam produced offspring, part of which had black spots and part of which entirely lacked black. The one exception farrowed only one pig.

Two F_1, three heterozygous F_2 and three other heterozygous black-spotted sows were back-crossed to Duroc-Jersey boars. Table II shows the occurrence of black in these back-crosses

This ratio is a fair fit for the monohybrid hypothesis.



TYPES IN LATER GENERATIONS

Figure 8

Four types are shown, reading from the top: Cream, red, intermediate spotting, predominantly black spotting. The cream and the red pigs are littermates, from a spotted F_2 sow bred back to a Duroc-Jersey boar. The red is as dark as that of a pure-bred Duroc-Jersey. The other two individuals are straight F_2's. The intermediate had a sandy body color which tended to fade with age, while the body color of the other was white, with just a trace of red.

Two litters of pigs were produced from two back-cross, heterozygous black-spotted sows when bred to an F_1 boar. Two litters were raised from one F_1 sow (Poland-China male × Duroc-Jersey female) when bred to the F_1 boar. Also four F_3 litters were raised from heterozygous spotted parents. Table III shows the occurrence of black among the progeny of these eight additional litters from the mating of heterozygous parents.

When the color data of these are combined with the first list of F_2 progeny (Table I) we get the totals in Table IV, showing the occurrence of black in all of the progeny of heterozygous males mated with heterozygous females.

Some evidence may be gathered from the data as to the proportion of homozygous to heterozygous dominants in the F_2 generation. On a single factor hypothesis this should be 1:2. Altogether, eleven litters of pigs were

TABLE I. DISTRIBUTION OF BLACK IN F_2

	Black present	*Black absent*	*Total*
Actual	107.	31	138
Expected (3 to 1)	103.5	34.5	138
Deviation	+3.5	—3.5	

TABLE II. OCCURRENCE OF BLACK IN BACK-CROSS OF HETEROZYGOUS BLACK TO PURE RED

	Black present	*Black absent*	*Total*
Actual	33	43	76
Expected (1 to 1)	38	38	76
Deviation	—5	+5	

TABLE III. OCCURRENCE OF BLACK IN ADDITIONAL F_2 HETEROZYGOUS × HETEROZYGOUS MATINGS

	Black present	*Black absent*	*Total*
Actual	45	16	61
Expected (3 to 1)	45.75	15.25	61
Deviation	—.75	+.75	

TABLE IV. DISTRIBUTION OF BLACK IN ALL MATINGS OF HETEROZYGOUS MALES TO HETEROZYGOUS FEMALES

	Black present	*Black absent*	*Total*
Actual	152	47	199
Expected (3 to 1)	149.25	49.75	199
Deviation	+2.75	—2.75	

produced by nine black-spotted F_2 females. Seven of these litters were sired by a black-spotted F_2 boar. The other four were by a red boar (pure-bred Duroc-Jersey). The progeny from six of the nine females tested included both black-spotted and non-black individuals in each litter. This test proves definitely that the six sows were heterozygous. Two of the other sows had respectively seven and eight pigs by the black-spotted boar. All of these pigs were black-spotted. The remaining sow raised one litter of four by the black-spotted boar and one litter of nine by the red boar, all of which were spotted. This indicates that these three sows may be homozygous for black. The black-spotted F_2 boar used is shown by the above data to be heterozygous. Thus nine females and one male of the F_2 generation were tested, and six of the females and the one male proved to be heterozygous and three of the females homozygous for black. This is a ratio of 3:1, which is very close to the expected 1 :2.

Discussion

In analyzing the preceding data, we have assumed a single pair of factors to be responsible for the difference between presence and absence of black. This working hypothesis fits the facts very well in the straight F_1 and F_2 generations, but deviates somewhat in the backcrosses.

If we were to assume that the multiple factors which govern the amount of black when present also determined whether black would be present or not, black would represent one end of the frequency curve of F_2 and red the other end, with practically all F_2 individuals showing some black. We might assume that some of the self-red individuals genotypically carry black spotting factors, but have missed getting it phenotypically in embryonic development. This kind of a situation has been demonstrated by Wright[3] in tri-colored guinea pigs. This would require two further assumptions in these pigs, both of which seem unlikely. The first assumption is that the phenotypic loss mentioned has taken place in almost 25 per cent of the F_2 generation, and the second, that large numbers of self-red swine of the Duroc-Jersey breed carry black-spotting factors. It seems logical to believe that there is a single factor difference between the black of Poland-Chinas and the red of Duroc-Jerseys. Besides this, there are probably multiple factors regulating the amount of black when present. This being the case, it may be possible, where black is extremely restricted, to obtain a self-red strain which still carries the factor for black, but it seems unlikely that this is the situation in the Duroc-Jersey breed.

Summary

A Duroc-Jersey male was crossed with several Poland-China females. One mating of the reciprocal cross was made. One hundred and thirty-one F_1 individuals were produced. All of the F_1 individuals had some black. The area between the black spots of the F_1 progeny varied from "white" to red.

One hundred and thirty-eight F_2, 76 back-cross, and 61 F_3 (heterozygous $\times$ heterozygous) and other heterozygous to heterozygous, progeny were produced. The ratios of black to absence of black conform very closely to the expected, on a single factor hypothesis.

Conclusions

1. Poland-China black is due to a single pair of dominant factors.

2. When black is present, its amount is regulated by other factors, probably multiple factors.

3. Probably several factors are involved in the intensity of red, but our stocks were too heterozygous to attempt an analysis of these from the data accumulated.

Literature Cited

1. Lush, J. L. 1921. Inheritance in swine. *Journal of Heredity*, Vol. 12, pp. 57-71. Illus.

2. Wentworth, E. N. and J. L. Lush. 1923. Inheritance in swine. *Jour. Agr. Research*, Vol. 23, pp. 557-582. Illus.

3. Wright, S. 1917. Color inheritance in mammals. V. The guinea-pig. *Journal of Heredity*, Vol. 8, pp. 476-480.

4. Wright, S. 1918. Color inheritance in mammals. VIII. Swine. *Journal of Heredity*, Vol. 9, pp. 33-38.

THE STORY OF THE MICROBE HUNTERS

A Review

Microbe Hunters, *by* Paul de Kruif, Harcourt, Brace & Co., New York, illustrated, pp. 363, price $3.50.

Frank Meyer in the heart of China stopped at a primitive little inn, the proprietor of which pointed with great pride to inscriptions on the walls left by the only two travelers who had before him ventured into those parts, an Englishman and a Frenchman. The Frenchman's encomium said: "This is the house of the thousand bugs (mille punaises)," and so Meyer found it. With equal truth this book might be called: "The book of the thousand thrills!" If there is not one on every page, there are some pages with half a dozen. Here is one of them:

> That is the way he went at his work. Without hardly so much as a how-d'ye-do to the officials and authorities, Bruce dragged Castellani back to the laboratory; in a few hours the microscopes were unpacked and set up; in a little while the doomed sleeping sickness patients began to be carried in: shortly after that the heavy needles were jabbed into their spines; the centrifuges whirred; Mrs. Bruce and Nabarro and Castellani were at their lenses, and the sergeant cleaning slides and glassware. There they sat, in this small room on the Equator, squinting down the barrels of their microscopes at a succession of gray nothingnesses.
>
> A bellow from Bruce: "I've got one!" The rest crowd around, squint in turn, exclaim as they watch the writhing trypanosome poke his exploring whip about in the gray field of the lens. Then they go back to their places—to shout discovery in their turn. So it went, from breakfast till the swift dusk of evening. In every single sample of spinal fluid from each one of his more than forty sleeping-sickness patients, Bruce and his slaves found those trypanosomes.

The book is as entertaining as a novel and worth a hundred such as are now blurbed to the sky line. There is not a dull page in it. It makes great men and important discoveries picturesque, interesting and *alive*. The author has a rare gift of evocation.

The first part of the book tells of the discovery of bacteria, of their relation to the fermentations, and of the destruction of the myth of spontaneous generation. This is done by means of sketchy biographies of Leeuwenhoek who first saw them, of Spallanzani who maintained that they never originated *de novo*, and of Louis Pasteur, who amplified in a hundred ways and placed on a solider basis all that had preceded him.

The rest of the book tells how scientific men endowed with imagination and a proper degree of obstinacy, discovered the cause of various widespread and terrifying diseases, or determined how they are transmitted, and found remedies for the same, mostly, be it understood *and remembered*, by means of experiments on the lower animals. This part deals with Pasteur and the mad dog; Robert Koch, tuberculosis and cholera; Roux and Behring, diphtheria; Metchnikoff and phagocytes; Theobald Smith and Texas fever; David Bruce and the Tsetse fly sleeping sickness of Africa;

Ross and Grassi on the transmission of malarial fever; Reed, Carroll and Lazear and those brave volunteers who enabled them to show how yellow fever is, and how it is not, transmitted; and finally, Ehrlich and his magic bullet, the remedy for syphilis.

This book holds the reader's attention from cover to cover. It is also remarkably free from serious errors. Its principal fault is that it is written too much in the language of the street. Occasionally one feels also that the dramatization is forced and that a little more restraint would improve it. The least interesting chapter is that devoted to Metchnikoff and the most interesting chapters are those dealing with Robert Koch, with David Bruce, and with Walter Reed and his colleagues of our yellow fever commission.

As a corollary to the yellow fever chapter there should have been, I think, a little about Noguchi's work and that of the International Health Board of the Rockefeller Foundation under whose philanthropic activities yellow fever has practically disappeared from the west coast of South America where it was formerly endemic and took many lives every year. In Guayaquil, for example, which was a pest hole, there has not been a case of yellow fever in six years because all the breeding places of the mosquito carrier have been hunted out and brought under control.

This book shows Paul De Kruif to have a copious vocabulary, much imagination and insight, picturesque language, dramatic expression and wide reading. Beyond any doubt he could write a better novel than Sinclair Lewis to whom he furnished the medico-bacteriological parts of "Arrowsmith." If anyone doubts my judgment let him get this wonderful book and read it.

Erwin F. Smith.

The Good Life

WHAT I BELIEVE, *by* BERTRAND RUSSELL. Pp. 87. $1.00. Dutton & Co., New York, 1925.

This short and entertaining essay by a well known philosopher appears in a series of booklets, The Today and Tomorrow Series, devoted to a discussion of modern science in its relation to the life and society of today and the future. In it Mr. Russell sets forth his ideas of "man's place in the universe and of his possibilities in the way of achieving the good life" which is to be "inspired by love and guided by knowledge." The failure of society to lead such a good life under its present systems of morality, religion and patriotism is due not to any inherent incapacity of men for being and doing good but to their docile subjection to an age old and irrational system and chiefly to ignorance. Science, Mr. Russell believes, can give men knowledge, and freedom and security to be achieved through knowledge will provide that self control and sympathy wherewith knowledge must be tempered. The author is hopeful. He believes in the power of science and in man's ability and desire to use this power for the general good. One feels his faith in the infinite educability of man, in the amenability of human nature to control, and in the benevolence of man's desires, to be at times a little too confident, yet he is neither a perfectibilist nor a utopian. He is first of all a philosopher who does not preach nor exhort, but relies as does a good teacher on reason and common sense, salted with wit, and spiced with keen but never bitter criticism. A stimulating half hour awaits all those who are interested in the relation of science and human life.

L. C. D.

CONSTRICTED EARS OF MAIZE

HARVEY J. SCONCE

A CONSTRICTED ear of maize was found in a field of Johnson County White Corn in 1916, on Fairview Farm by the author. The ear was about normal in size, nine inches in length, and with the kernels perfectly formed on the butt or large end of the ear up to half of the length, but at this point a peculiar constriction had occurred. The development was arrested, as if some powerful influence had gripped the ear at the small end, and stopped all growth shortly after the kernels had been pollinated.

The kernels of the constricted end of the ear are dwarfed and undeveloped, about one-half the size of the kernels of the other portion of the ear. The cob, however, is normal from end to end. The variation thus appears to be due to the incomplete development of the kernels on the small end of the ear.

For fifteen years preceding the date when this variation was found, the desirable characteristics of maize had been under the constant observation of the author. Exhaustive experiments had been conducted with such characters as length of ear, the circumference, number of rows of kernels, shape and length of kernels, and kernel composition, to determine if any relationship existed between these various characters, and yield and time of maturity.

By an experiment, over a period of fifteen years of two varieties of standard market corn, it was shown with some degree of conclusiveness that the long starchy kernels were much later in maturity than the short and higher protein kernels, that the smooth kernels gave the highest yield, due possibly to the fact that they were more disease resistant, rather than directly to inherited qualities. Nevertheless, it is quite apparent that the smooth ear is much earlier in maturity, less susceptible to disease, and produces a higher yield of market corn, than the rough ear of larger proportions.

In the matter of length and circumference of ear the results are not so clear. It is evident however, that the eight, nine, and eleven inch ears do not give the general average yields that the ten-inch ears do, although the variation in yields of the nine, nine and a half, ten, and ten and one-half ears are very slight.

With the belief that maturity and yield are heritable, but are greatly influenced by combining certain desirable characters, as length of ear, type, and composition of kernel, height of ear on the stalk, with disease resistant qualities, the possibilities of an interesting breeding experiment at once appealed to the author, when he discovered this unusual constricted deformed ear.

This seemed to be a case of arrested development; it might be due to a diseased condition of the stalk, or it might be a recessive character that was capable of reproduction. If the latter, it would be most interesting because of the application that could be made to the human race, illustrating cases of arrested development in the children of our country. Many children are so unfortunate as to have some calamity befall them, causing a similar condition, as through disease, or heritable characters in past generations, a deformity makes its appearance during the growth of the child, that completely ruins its life.

In order to determine the behavior of the two types of kernels on this constricted ear, the normal kernels were planted in one row, while the dwarfed kernels were planted in another row, during the spring of 1917.

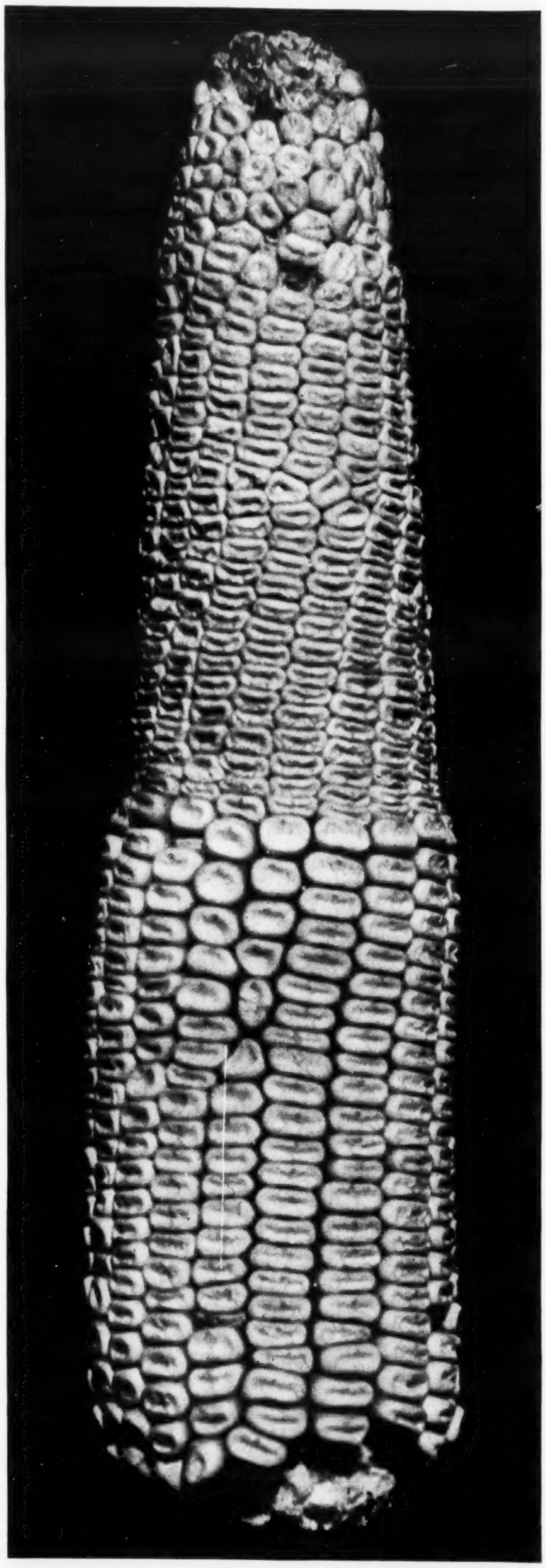
The germination of the normal kernels showed greater vigor than the dwarfed ones, but aside from this fact, the growth and development of the various plants was much alike. Many of the ears of both rows were selfed this year and planted in isolated plots during the year 1918, and while no deformed ears were produced from the normal strain of the original ear, yet several variations of constricted ears were grown from the dwarfed kernels of the 1916 planting, as shown in the accompanying photograph.

Owing to the fact that this variation was not sufficiently clear cut to get the exact count on the classification of the ears, the Mendelian ratio and segregation were not determined with any degree of accuracy.

The selfing was continued year after year, and it was soon noted that there was a marked tendency for this constriction to be diseased, so much so that quite a proportion of the ears at husking time were partially, or totally destroyed by the Fusarium and Diplodia diseases. This is illustrated in Figure 10, showing the normal end of the ear in good condition, while the abnormal part is badly diseased. In many cases it is totally decomposed, so much that when the husk was removed the upper portion of the ear, the constricted part, would fall apart, as the cob at the middle of the ear was completely gone.

Up to this present date—1922—the corn was always a continuation of the Johnson County White, but this season some of these white constricted ears were crossed with a beautiful crimson ear, perfectly normal in every respect.

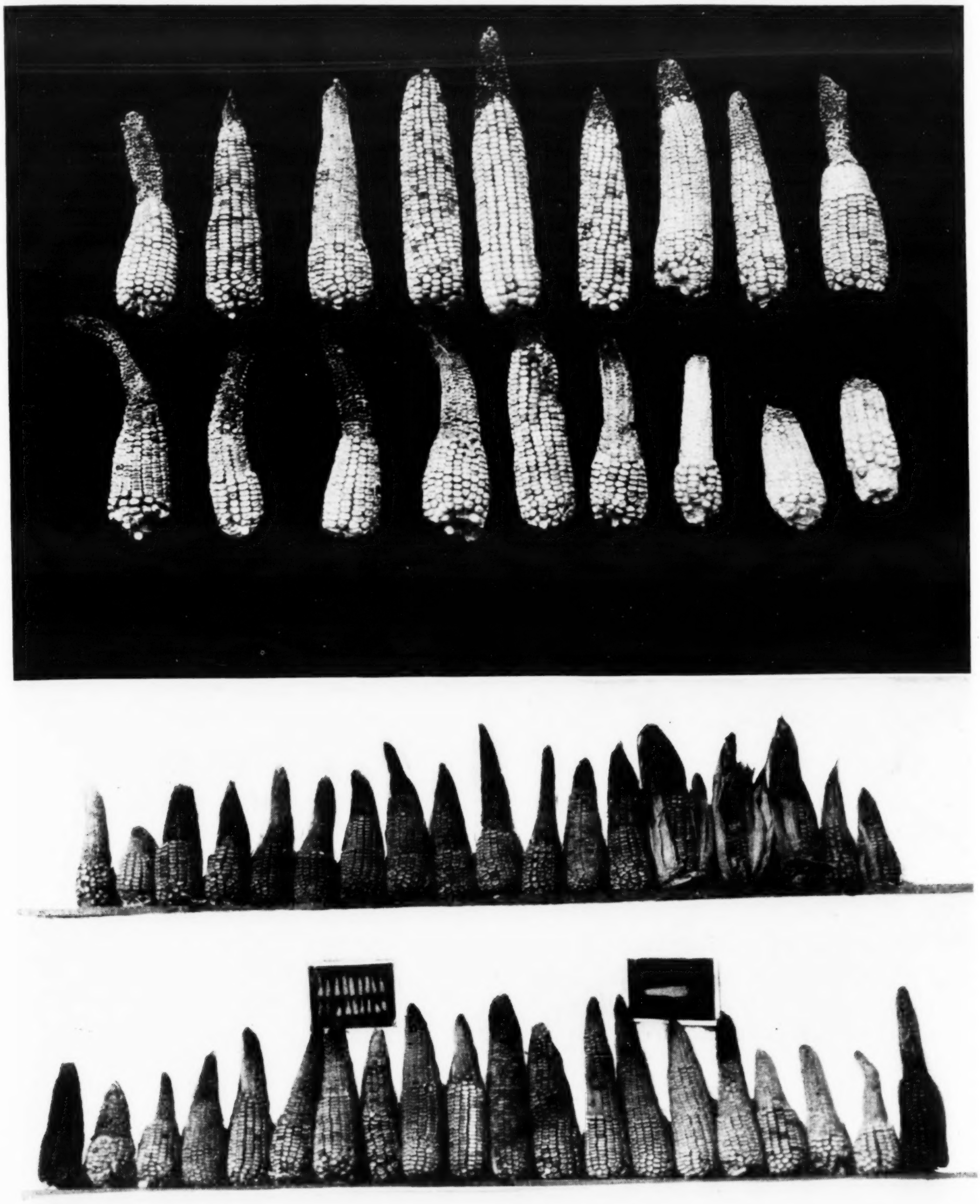
In 1923 the hybrid ears were planted; the plants selfed, and in 1924 the F_2 population showed two crimson constricted ears, and are shown at either end of the bottom row in Figure 10. These two ears were planted this past season and the results are shown in Figure 11. It is quite appar-



ORIGINAL CONSTRICTED EAR

Figure 9

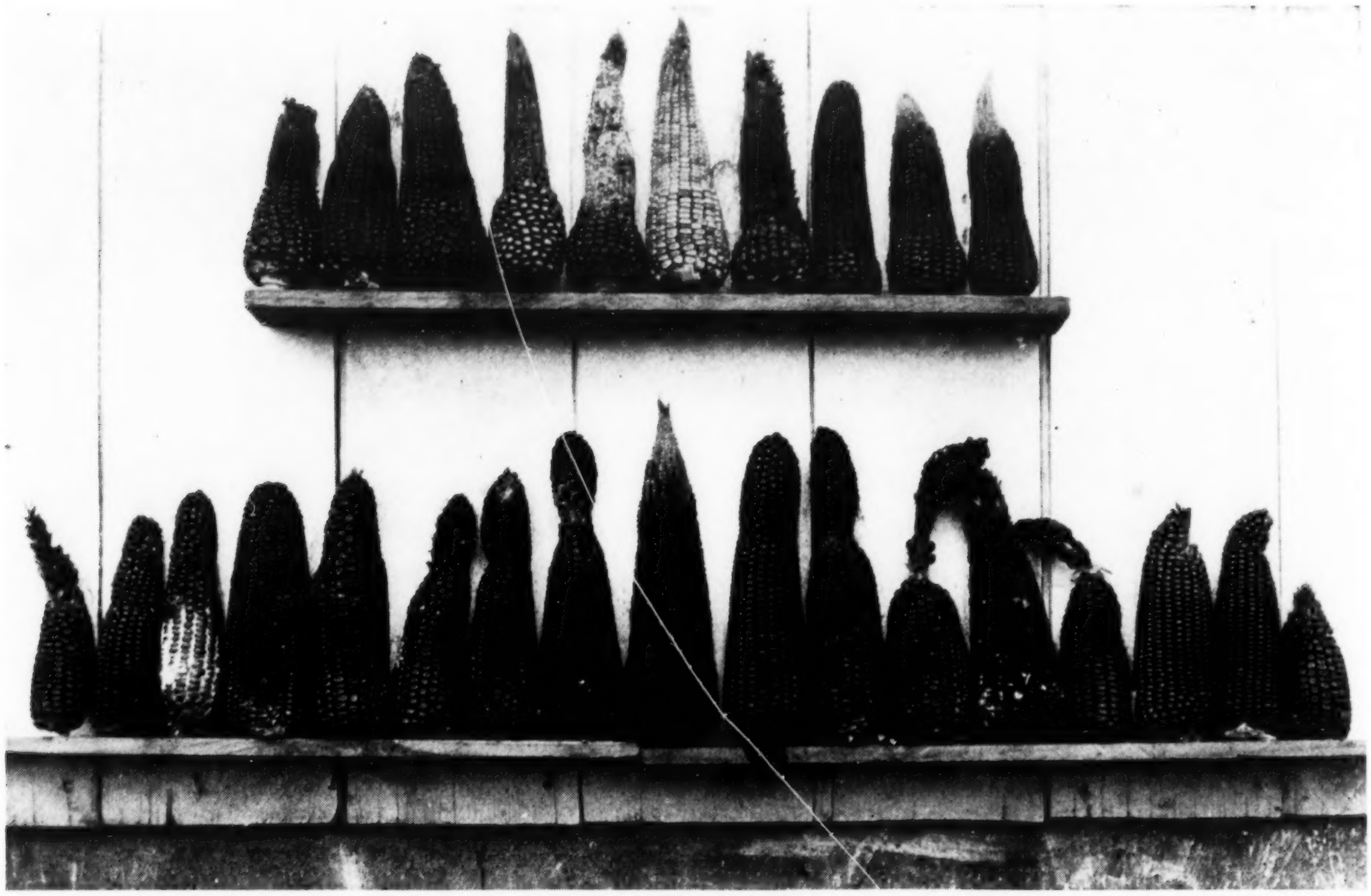
This ear was found in 1916 in a field of Johnson County White Corn.



PROGENY OF THE CONSTRICTED EAR

Figure 10

Above are shown some of the constricted ears produced by the seed from the original ear. Below is shown the progeny in the fifth generation. Two generations before this a crimson strain was combined with the constricted, and two crimson segregates are shown here, at the ends of the lower row.



CRIMSON CONSTRICTED EARS

Figure 11

The ear from the 1925 crop shows a combination of crimson and constricted. While no progeny counts have been made it is evident that this character is heritable, and that it can be combined with pericarp color, and doubtless with other characters.

ent that these F_2 segregates have bred true for constriction.

A great proportion of the deformed ears was badly diseased even though new blood of the healthy crimson ear had been introduced into the deformed strain. It was discovered that there were deformed ears that were not diseased in the least, so the deduction is that in this cross there are four classes: (1) normal, non-diseased; (2) normal, diseased, (3) deformed, non-diseased; (4) deformed, diseased. Owing to the conditions at the time of harvesting, the definite ratio of each class was not determined.

The conclusions of the author are that this constriction is heritable, that it is quite susceptible to disease, as are most deformities of maize. Further, that only the dwarfed kernels of the original ear showed a tendency to produce a deformed progeny, but during the following years all the kernels of the selfed constricted ears, whether normal or dwarfed, produced alike, the constricted ears. That it will combine with pericarp color, as possibly it will with any other character.

The disease-carrying human individual combined with a deformed character, mind and body, steeped in sin, as are the morons, dementia-precox individuals, the criminally insane, and other undesirables of the race, are humanity's greatest menace and problem. Their undesirable characters are transmitted to future generations, just the same as the color, the disease, and the deformity of the ear of corn, so that when the blood of the normal individual of the race is contaminated with that of this undesirable strain, the normal is ruined for all progressive purposes of character building, in the plant or animal world, or in the building of a Nation.

CAUSES OF WAR

A Review

MILLER'S book* is a sociological study of present-day group conflicts (those between nations, classes, etc.) and a discussion of the ideas and sentiments which are necessary to bring from such conflicts order and progress instead of revolution. The viewpoint is quite different from that of the usual student of Science. Geneticists cannot accept entirely some of the author's conclusions, yet his book can hardly help being of deep interest to them.

The larger portion of the book is devoted to an analysis of group conflicts in the various countries of the world; a couple of chapters to the "Myth of Superiority" especially as regards the white and other races; and lastly two chapters to Americanization and patriotism.

Group loyalty, Miller believes, has always been essential to the existence of man. Throughout the ages there has been a constant selection of the socially inclined individuals in whom group loyalty is strong. Finally, from such groups have developed nations and nationality based on common blood, common ancestry, history, religion, or language. The conflicts of the world are between every possible combination of such groups.

Any domination of one group by another politically, economically or culturally is "oppression" which leads to a pathological state of mind on the part of the oppressed. Freedom is merely the privilege of self-determination and oppression a mental disorder which results from any domination of one group by another. The United States is interested because within its borders are 20,000,000 people, either immigrants or children of immigrants, with a pathological state of mind as the result of previous or existing oppression in Europe. To make of these peoples one, all the wrongs, real and imagined of Europe, must be understood—why the Irish and Poles are intensely Catholic, the Czechs free-thinkers, the Jews so united. Where the followers of one religion dominate other groups the oppressed tend to become of another religion than that of the oppressors. In the same way where an attempt is made to force the language of the dominant group on the others, the oppressed often use language as a symbol of patriotism. Thus is explained the survival and revival of Polish, Irish, Czech, Finnish, and other languages.

The various countries where pathological states of mind exist are taken up and an attempt made to understand the cause in each case—Poland, Czecho-Slovakia, Hungary, Servia, Jugo-Slavia, the Jews, Ireland, French Canada, India, Mexico, and Korea.

The Myth of Superiority

Geneticists, however, will be especially interested in the discussion of "The Myth of Superiority":

> Our own time has seen the fall of the idea of the divine right of kings, of the divine right of class and of male superiority.—The divine rightists can now seize on biology, the process of evolution and the preeminence of the fittest to establish social position which formerly was only secured by aristocratic birth.—The myth of racial inferiority—or racial superiority—differs from most angles in the modernness of its origin. Two hundred years ago it did not exist. A modern culture group results from the fortuitous possession of organization, accumulated wealth, momentum, machinery and prestige; each of these as a factor multiplies the significance of both the individual and the group, incalculably more than any possible variation in natural endowment could do.
>
> Certain ideas and the organization of Government gave the white race a start

*MILLER, H. A.—Races, Nations and Classes. Pp. 196. J. P. Lippincott Company, 1924.

some four hundred years ago that enabled it to expand to the uttermost parts of the earth, but there is not an iota of proof in this expansion that therefore the white race is better endowed than those whom they have subjected.—"Race is the cheap explanation tyros offer for any collective trait that they are too stupid or too lazy to trace to its origin in the physical environment, or historical conditions (Royce)"—

Hundred percent patriotism and confidence in Nordic superiority are the two most dangerous ideas in the world to-day, because they lead in exactly the opposite direction from that which civilization must take if it is to survive. The fundamental objections to these ideas are, first, that they have no basis in fact, and second, that the emotions which they organize, have far-reaching and disruptive consequences.—

The vocabulary of science has been appropriated and its methods prostituted to prove, namely; their moral right to keep what they want to keep. The most fruitful medium for this method has been intelligence testing. We have developed a large crop of pseudo-scientists. They are of two sorts; first, those who have genuine scientific standing in one field—say biology, psychology, or education, and who go into other fields to make generalizations such as they would not dream of making within their own fields.—The other kind of pseudo-scientists might well be called half-baked scientists. They have, in a few hours or weeks, learned to give Binet tests, and they have as much blatancy and confidence as the advertisement of patent medicine panacea.

Intelligence testing is one of the most promising fields of research now before us, but it is also liable to as much mythical use as ever came from a supernational religion. . . . . The really significant mental processes which may mark fundamental differences between peoples are not yet subjected to controlled psychological experimentation. Granting that one race may show a larger. number of individuals lower in the psychological scale than the other, every series of tests will show that the number of either race who fall outside the curve plotting the cases, either above or below the other race, is a very small percent. In other words, instead of drawing a line between races, psychological comparison demonstrates, by the overlapping, similarity instead of difference. . . . . It is most unscientific to compare *average* intelligence quotients. . . . Further, since there is slight relation between the orginators and possessors of culture, it may often happen that the culture of the dominant race has been secured from the "inferior" race or culture. . . . In the region of the Mediterranean most of European culture was developed. The Nordics, after appropriating the contribution, repudiate the creators of it. . . . . A revolution will as surely follow the application of cultural dominance based on intelligence quotients as that based on divine right of birth or on wealth. Unquestioned superiority of the white race is the honest conviction of the great mass of those in it. This is not injustice nor immorality but illusion. The other races have had similar illusions about their superiority. It is easy to think because two groups are juxtaposed, as are the negro and the Caucasion in America, that they are living in the same cultural environment, but this ignores the psychology of groups, and the method by which cultures are transmitted and transformed. . . . . The fact that the negroes have lived in America and have not all reached the same standards of life as the white people is no measure of their inability to do so and must be credited to culture inheritance rather than race inheritance. . . . There is no evidence that the superiority of any existing dominant group is based on anything but an accident, and any attempt to maintain that dominance by reason, is merely the rationalization of a myth.

So-called race questions are considered by Professor Miller as essentially sociological rather than biological. "The world of modern intellectual life is in reality a white man's world. Few women and perhaps no blacks have ever entered this world in the fullest sense." (Thomas).

The negroes in the United States number about three times as many as all the Irish in Ireland, and equal or exceed in numbers all of the new Nations in Middle Europe except Poland. They number more than all the Jews in the world. . . . . There is no easy solving of the negro problem. . . . . The negro, himself, needs perspective to enable him to see that his case is not a unique one among mankind, but one that has been common throughout history.

In the quotations given, Professor Miller has used the method of denying the truth of more or less common ideas without offering evidence — a method hardly scientific. Yet he has raised questions and issued a warning that should not be lightly passed over. Are there real scientific grounds for thinking that there are hereditary differences between individuals and between races and nations? If so, how

important are those differences and what shall our attitude toward them be?

We assume that Professor Miller means by a dominant group in this discussion one whose cultural values are superior to another. Are such dominant groups inherently superior to other groups? It is impossible not to believe in the inheritance of physical characteristics by individuals and by groups or races—inheritance of color, stature, features, defects, etc. The same can be said of certain types of mental deficiency. Is there then any reason to believe that mental ability above the average is not inherited? All the evidence points to its being inherited. We have, of course, no standard of superiority. Thus, a tribe of Indians in the highlands of Central America who cooperate to the fullest extent, who deal justly and who are intelligent may, to some extent, be superior to a dominant Spanish group. Moreover, we do not have clearly in mind just what kind of abilities will be most important to the world one or ten centuries hence.

Actually the question is, "Is it correct to say that the white groups are superior if their average ability is above that of other groups, and if so, is their average *actually* above that of other groups?" It is scientific to compare average yields of different varieties of wheat, oats, and other plants and such comparisons have been of great economic advantage to the world. We see no reason why it is not scientific and may not be of great advantage to compare average intelligence quotients. Very few psychologists or geneticists believe that present intelligence tests are a final measure of inherent differences between people, yet such tests have proved useful. Within certain limitations they do measure some of the differences between peoples. As the tests are improved they will measure more and more of such inherent differences. The geneticist is not deeply concerned as to whether the white group is inherently superior. The test of time may prove or disprove this. To him the important problem is how to insure the survival of the ablest strains in all races. Because of the actual danger of the permanent loss of such strains he is especially interested in them. Since this is or may be a problem of great importance to other groups we think they have the same interest in it that we have, and we see no danger in discussing and investigating it. Only good is likely to result.

The conflict of class is discussed in a manner similar to that of race groups but still less convincingly. Thus in discussing quantity production Professor Miller presents the stock statement that it necessitates the doing of the same thing over and over with the least possible variation. This repetitive type of labor no man can survive as a free human being unless he identifies himself with some form of activity other than his work. "Work itself, then, is ceasing to have any attraction and to be performed solely for the pay received." He forgets the drudgery of life in China, India and other countries without quantity production. He forgets, too, the lightening of the drudgery of women by machine production of cloth and clothing, bakers' goods, automobiles, vacuum cleaners, electric lights, etc.; he forgets, also, the continual replacement of the labor of drudgery by that of the skilled mechanic who keeps machines in order; and he forgets the larger life made possible to the many.

Professor Miller's viewpoint of the relations between labor and capital seem almost pathological. He believes that labor and capital in their mutual antagonism are drawing apart, that the lines are being drawn closer, and that to labor "capital has become as barren of decency and morality as the national enemy in time of war. In the field of production there is now a race between the adoption of methods harmonious with self-respecting freedom, and the chaos of revolution." No one can review the closer relations between labor and capital, the decrease in violence on the part of both and their increasing

consciousness of responsibility without concluding that great progress has been made in recent years. To the reviewer most of the current difficulties between labor and capital seem closely connected with pathological states of mind resulting from the oppressions of Europe and this makes our problems, as the author well says, as broad as the problems of Europe.

His concluding chapters on "The Paradox of Americanism" and "Proportional Liberty" are especially fine.

George M. Darrow.

Biological Metaphysics

ABERATIONS OF LIFE *by* JAMES CLARK MCKERROW. Pp. vi + 108. Price $2.00. Longmans Green and Company, London and New York. 1923.

The author makes a grave oversight in waiting until the second half of the book to "warn the reader he must rely on his own intelligence and goodwill to get our meaning, rather than on the exactness of our terminology." Only by skipping the more involved passages were we able to arrive at this request for our cooperation.

The book is a sequel to the author's "Appearance of Mind," and evidently marks the disappearance of that important faculty. It might be described as a Shavian metaphysical treatise, flavored with loosely used biological terms. Heredity, fortunately, does not take a large place in the discussion. The chromosome theory is disposed of in the following illuminating passage, which gives an excellent example of that vagueness of which the author is so justly proud:

"A muticellular individual is one in which multiplication by fission ("cleavage," "segmentation"), occurs before the zygote (fusion of two asexual individuals) has separated into its constituent elements; each somatic cell is of a double nature. The physio-chemical conditions of this variation (as of conjugation between unicellular organisms), is not our concern. (It is the obvious guess that electricity has a finger in the pie.) Nor are we concerned with its biological mechanism, with chromosomes and allelomorphs. We may remark, however, that we cannot approve the motion that the chromosomes are carriers of specific characters. This is the fallacy of our spatializing intellects, so prone to confuse occasion and cause, and to single out *one* condition as *the* condition; so incapable of regarding history as a condition."

Some of the less involved passages partially reward one for struggling through the difficult portions. The final paragraph is not concerned with eugenics, nor as far as our poor intelligence goes, is it concerned with the question of aberations or anything else, but taken by itself it appears to carry an idea worthy of the eugenist's consideration:

"A love of Goodness, Beauty and Truth in the abstract, or even a sensual or aesthetic love of them in the concrete, is like Faith without Works—dead. A husband who loves his wife, a wife who loves her husband are already 'saved.' They have embodied Goodness, Beauty and Truth, and loved them. With all respect to the saints who supposed only as bachelors could they be saved, we suggest that it is only the bachelors who need saving."

It is to be hoped that psychology will some day undertake to explain to us what goes on in the mind of a publisher when he puts out a book of this kind. By giving currency to such a treatise, publishers are responsible for muddling the minds of the people who undertake to read it. Biology is so complicated that specialists get into difficulties trying to be understood, and such a book only adds to the confusion, for it is not biology, although the layman may be led to think that it is.

R. C.

SHARP'S CYTOLOGY REVISED*

A Review

THE large number of important articles by prominent cytologists, appearing in the last decade, have tended to bring this branch of biological science into the limelight. A desire on the part of the author to incorporate the outstanding topics of the more recent papers and to shift the attention of the reader from the individual cell to the living system as a whole—from the cell theory to the organismal theory—seem justifiable reasons for the rewriting of "Introduction to Cytology," after it has rendered a mere five years of public service.

This second edition has been enlarged by over 100 pages. The additions and changes to the text have been such that the book may still be spoken of as well balanced. If any one section has been unduly favored it is those chapters that draw our attention to the chromosomes and their relation to heredity and development. We can not attach blame for this, if any is due, to the author alone for he is only emphasizing those subjects that are receiving the greatest attention of his colleagues. They will have to share the responsibility for this stimulation of interest in those minute bodies, the chromosomes, upon which thousands of eyes and microscopes are focused.

Many commendable changes in chapter subdivisions have been made and the student will find it necessary to familiarize himself with what appears from many sections to be a new book. In a few cases the outstanding changes are in chapter titles. "Meioses," "Gametogenesis," and "Syngamy" supplant the more familiar terms "The Reduction of the Chromosomes" and "Fertilization."

These latter changes introduce the question of terminology. There seems to be an effort to avoid as far as possible synonyms. For example, the phrase "pollen-mother-cell," a descriptive synonym for microsporocyte, no longer is found in the text but such elimination has not been followed consistently. The phrase, "meiotic divisions" is introduced, in good standing, but soon changes to "meiotic meioses," and the two terms are used interchangeably throughout the remainder of the book.

The reviewer is struck by the following definition of hybridization: "The sexual union of any two protoplasms differing in hereditary potency . . . constitutes an act of hybridization in the fundamental sense (367)"; and of heterospory: "One may designate as *heterosporic* any species with spores of two kinds as regards sexual tendency, whether they are alike or unlike in size, and whether they are produced as members of the same quartet or in separate quartets (424)." By broadening the latter definition to include molecular and atomic differences the contrasting term homosporic probably could be done away with. A hybrid as above defined becomes so inclusive that the term seems superfluous. The few organisms that are not the outcome of such a union are usually specifically labeled and so the term organism as generally used and hybrid become close synonyms.

A reviewer of the previous edition pointed out certain inaccurate statements. Under this heading might be included the following statements: "The outer three megaspores disintegrate while the innermost enlarges to form the 'embryo sac' (246)." In the following paragraphs the author describes "many variations" in embryo

*Introduction to Cytology, by Lester W. Sharp, Ph.D., pp. 581, 210 text fig. N. Y., McGraw-Hill Book Co. 1926.

sac formation, but one must look to others to find that the third or even the second cell of the linear tetrad may develop to form the embryo sac. The description of "Figure 196" is perhaps unobjectionable to all but the anti-evolutionists, who will hardly sanction the inclusion of opossum and monkey chromosomes under the general heading "The chromosomes in man."

The controversy over the appearance of the chromosomes at diakinesis, originating from Miss Carothers' criticism of statements made in the first edition, is closed by a reiteration of the earlier view of the author in the following words. "In many cases, particularly in animals, both the plane of synapsis and that of the splitting may be visible in the prophase of the first mitosis giving tetrad chromosomes . . ." (253). Figure 109 illustrates diagramatically what Prof. Sharp means by the statement. His view leaves both zoologists and botanists free to hold to their own convictions on this subject.

Cytology is a science that demands accuracy of the finest degree. It is to the illustrations that the critic turns to form hastily an opinion, favorable or otherwise, of any cytological publication. Little but praise can be said of the figures selected to illustrate the text—they are clear-cut, well-placed and sufficiently abundant to serve as charts for the reader in following the discussions that are almost meaningless unless accompanied by photographs, camera lucida and diagramatic drawings.

In his first edition Prof. Sharp seemed to have made an untiring effort to bring together cytological data from every source and weave them into a harmonious whole, thus insuring the reader a safe middle ground for reflection. The attempt to harmonize conflicting cytological views is not quite so evident in his second edition, and the author seems to have grown bolder with five years of experience, for in the light of research and reason he now leans to one and now to the other side in points of dissension thus incorporating in the revision more of his own individuality.

Students and teachers of Botanical Cytology will find the "Introduction" a vast storehouse of facts. These facts combined with the masterly presentation will hardly fail to stimulate the interest of the earnest reader along his favored line of study and research.

The more accessible form of the bibliography makes this text indispensable as a reference book. The elimination of titles in individual citations is unfortunate, but some such curtailing of space seems necessary in order to accommodate the ever-increasing volume of literature.

The world is more prone to criticize than praise. The reviewer would do a great injustice if attention is not drawn towards the author's sane viewpoint in the treaties on plastids and chondriosomes while the chapter on Golgi material opens a field that is still virgin for the botanist whose interest and efforts are turning in that direction. The concluding paragraph of the chapter on "Mendelism, Mutation and Hybridization" seems to be written for those who belittle cytology and the role it is playing in Biological research.

"Introduction to Cytology," as it appears in its revised form and from a botanical viewpoint, is an up-to-date text and reference book. The zoologist will not find it all-inclusive but the biologist will find it indispensable on his desk and in the classroom.

A. E. L.

NEWS AND NOTES

Where are the Best Honey Locust Trees?

Cash Prizes for Their Discovery

THE sugary pods of the beans of the Carob tree have been an important forage crop in Mediterranean countries for centuries. They have even been food for men as well. They were John the Baptist's "locust" and the "husks" of the prodigal son.

In India millions of bushels of the beans of the Babul tree are eaten each year by goats, sheep and cattle.

In the Western United States the mesquite beans are at times an important part of the food of game and domestic animals.

Here are some of nature's hints to a new agriculture and to constructive conservation of natural resources:

The gully is the greatest resource waster in the world because it destroys the soil, the resource for which there is no substitute.

We plow our hills and let them wash away while we try to grow corn, cotton and tobacco on the cultivated slope. In one of our southern states a county seat had to be moved because a gully ate up the townsite.

We need to have trees on our hills and crops on the trees—crops to harvest every year—*a tree crop agriculture*. We can have this by applying known science and a little common sense to some of our native and foreign trees. Enter this contest and you will help bring this to pass.

In the Eastern United States is the honey locust, a neglected tree of great promise. The tree is a legume. Its bean and pod have an analysis as good as that of many commercial grain feeds. Cattle eat some of them greedily. Some trees yield them heavily—many bushels.

Here is a possible crop of great value, both as a harvest and as a soil saver through letting tree roots stop gullies. We need to have the best wild trees with which to begin, just as the navel orange business began with finding *one* tree that happened to bear that kind of fruit. The JOURNAL OF HEREDITY offers $50 to find the best trees; $30 for the best, $10 for the second best, $5 for the third best and $5 for the fourth best.

The conditions of the award are as follows:

1st.—Send to the office of the American Genetic Association, 724 Ninth Street, N. W., Washington, D. C., a six-pound sample of the dried beans; contestant agreeing to send a second sample for further analysis if cost of shipment is paid. It is important that entire pods be included to show their size and shape.

2nd.—A photograph of the tree. Photographs should, when possible, contain some object, such as a human figure, which will aid in giving a realization of the size of the tree; but such figure should be beside, not in front of tree. It is necessary that the photograph should include the whole tree. If there are other trees growing beside it and hiding part of it these other trees should be included in the picture. Contestants may send photographs of as many different trees as they like.

With each photograph, a statement should be submitted telling all that is known about the tree, with reference to its age, the size and regularity of crop it bears, the quality of the fruit; the character of the soil and surround-

ing vegetation. If the tree is on private land, and likely to be destroyed, the fact should be mentioned. It will be helpful if contestants can tell to what extent the tree is subject to attacks by disease or insects. In short, the Council desires to gain as much information as possible about the honey locust trees of the United States; but it imposes as few hard-and-fast restrictions as possible, because of the varying conditions under which photographs may have to be taken, or under which they have been taken at some time in the past.

Photographs must be on glossy paper, not smaller than 4x5 or 3¼x4¼ inches, and must be of sufficient excellence to allow reproduction in the JOURNAL OF HEREDITY or elsewhere. Photographs in which the tree is so small that its details cannot be made out, cannot be considered.

All photographs submitted will become the property of the American Genetic Association, to be kept as a scientific record or used in any way that the Council may think desirable.

3rd.—Some proof of the bean-producing habits of the tree. This may be by a record of actual measurement of the crop or by means of a photograph that shows the crop on all or part of the tree.

4th.—Give the exact location of the tree, so that it may be easily found.

5th.—Those entering this contest agree to furnish to the Genetic Association two samples of cions from the contesting tree for experimental propagation, and if necessary to render such other assistance, without cost to themselves, as may be necessary to enable the Association to propagate the tree in question. The technique of propagation of honey locust has not yet been worked out.

Contest closes January 1st, 1928, so that trees bearing heavy biennial crops may have full opportunity to show performance.

In judging this contest the Council will take into consideration the size of the tree, the size of individual pods, their chemical analysis and feeding value, and the way in which the data and photographs are presented. It is not known how much diversity in size, yield, and other factors will be encountered, and with so many variables to take into account it is impossible to state in advance the weight each of these factors will be given in arriving at a conclusion.

The Council desires to acknowledge the kindness of Professor J. Russell Smith of Columbia University whose donation of $50 for prizes has made the contest possible.

Centipede Grass—A Recent Plant Immigrant

CENTIPEDE grass introduced from China by the United States Department of Agriculaure and tested several years at Tifton, Ga., Gainesville, Ga., and McNeil, Miss., promises to rejuvenate southern pastures, says H. N. Vinall, who is engaged in forage investigations. Several patches of the grass put out in hill pastures at these stations remained alive and green throughout the extremely dry summers of 1924-25. The new grass spread into the native grasses growing adjacent, giving every indication of being very aggressive and drought resistant.

The original small sample of seed came from the Province of Hunan in China and was known as Hunan grass. It was renamed by the late Doctor Piper because of its long surface runners or stolons, which in their numerous root connections with the soil somewhat resemble a centipede.

In its early growth the grass is protected from grazing animals by the closeness with which the runners and leaf blades adhere to the ground. After it becomes somewhat thick on the ground the leaves take an upward turn, growing on an average about 3 or 4 inches high. It makes an ex-

cellent pasture, is readily eaten by livestock, and remains green when the native grasses have dried up. It may also be used for lawns and golf greens.

The new grass may be propagated either vegetatively or by seed. However, the vegetative method is rather expensive and has the disadvantage of destroying the original patch from which the runners or stolons are obtained. New areas have been started at the three stations for supplying vegetative propagating material, but this source of supply will be quite limited for some time to come. Seed is not generally produced in quantities in this country, but plans are being made to locate a source of seed supply in China. When good seed becomes available centipede grass will no doubt become popular in the Gulf States for pastures, lawns, and golf courses.

U. S. Dept., Agr.

The House of Lords Discusses Birth Control

IN the House of Lords, Lord Buckmaster moved that the government be requested to withdraw all conditions imposed on welfare committees for the purpose of causing the withholding from married women of information when sought as to the best means of limiting their families. The control of birth was, he said, being extensively practiced by the well-to-do, and it was only among the poorest and the most ignorant that this knowledge had not permeated. In 1,000 married people under the age of 50, the birth rate for schoolmasters was 93; for clergymen of the Church of England, 100; for clergy of all denominations, about 102; for physicians and professional men, from 103 to 105; for skilled laborers, 153, and for unskilled laborers, 147. The standard of intelligence measured in the same way by examination of the children in the schools showed a decreasing scale of intelligence, which was in the exact inverse ratio to this standard of birth rate. The ministry of health provided money to assist welfare centers set up by local authorities, and to enable help and advice to be given to women expecting children, and to women who had young children. But an express prohibition prevented the information from being given as to the means by which future births might be prevented. Should that state of things, not merely in the interests of justice but in the interests of the race, be allowed to continue? Many people deplored the fact that birth control was exercised by the well-to-do, but it was no use weeping over lost battlefields. The reason was this: It was not, as some said, that women were selfish and men were worthless and sought nothing but pleasure, but it was a much higher thing—the increased respect for the sanctity of life, and the desire of people to see that their children were not pressed out by the illimitable competition of unrestricted numbers. It was inspired by the hope that parents would be able to make for their children a better place in the world than they themselves once filled. The better and more thoughtful people were limiting their children, and the sickly, the diseased and the underfed were reproducing at a rate nearly double that of anybody else. He asked that duly qualified physicians should not have their discretion fettered in giving advice. The idea that economically, under all circumstances, the population ought to be reproduced was long since dead. children were no longer to be treated as fodder for the production of big profits in the balance sheets of manufacturers. With regard to the argument that limitation of the birth rate meant descending in the scale of nations, he asked whether nations were

to be tested by their fecundity, like rabbits. It was not numbers that would win the next war, but brains. As to the deep-rooted religious objection as to limitation, he was unable to understand its grounds. It was said that the right course was for men and women to exercise self-restraint, and that abstention would solve the difficulty. The doctrine of abstention was a mischievous doctrine and would lead to the shattering of more homes than any of the causes they had heard denounced in that house.

For the government, the Marquess of Salisbury said that he approached the subject with great diffidence. In trying to remedy the sorrows with which they sympathized, they must be careful that they did not strike a vital blow at a work of service which was doing infinite good to the poorest class. Lord Buckmaster would strike a fatal blow at the maternity and child welfare movement. He was proposing to introduce into its purview a subject that was alien and in the highest degree controversial. Birth control might be useful and necessary, but the kind of people interested in the one movement were not necessarily interested in the other. It would profoundly shock a large number of those enaged in the voluntary work at child welfare and maternity centers if birth control were brought within their purview. The feeling on this subject was not confined to the Roman Catholic Church. It was shared by a large number of religious bodies. Supposing a large number of these voluntary workers who agreed with the Roman Catholic Church or with other churches on this matter should resign their work at the child welfare and maternity centers, what would become of this highly successful movement? For these reasons the government could not support the motion. Birth control was not confined to people whose health dictated that they should not bear more children. It was largely supported by women who probably could have children but preferred not to have, women who preferred their own ease.

In the discussion that followed, the lord chancellor pointed out that the house was not asked to express any opinion for or against birth control, or to say whether propaganda in favor of birth control should or should not be lawful. The resolution was not confined to women who sought advice on the ground of health: it extended the right to information to all married women. Moreover, it would empower a non-medical member of the committee to give advice. The minstry of health intended that in any case in which it appeared to the committee that further conception might be dangerous to health, the person should be referred to the physician attached to the welfare center. The advice must be medical. Lord Buckmaster then said that there was a misapprehension as to the purpose of the motion. It was never his intention that information should be disseminated broadcast through unqualified people. The government did not object to the motion if he would only introduce the words "through the medical officer." He was quite willing to do so, but it was clear from the trend of the debate that the motion was opposed not on this ground, but on the ground that the information ought not to be distributed at all. He was perfectly willing that there should be medical control, but he was not willing that a physician should be put in charge of one of the most delicate and important medical subjects and that then his discretion should be fettered. The motion was carried by 57 votes to 44.

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